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EDITORIAL NOTE.

The object of this Journal is to record the psychological work of a scientific, as distinct from a speculative character, which has been so widely scattered as to be largely inaccessible save to a very few, and often to be overlooked by them. Several departments of science, sometimes so distinct from each other that their contributions are not mutually known, have touched and enriched psychology, bringing to it their best methods and their clearest insights. It is from this circumstance that the vast progress made in this department of late years is so little realized, and the field for such a journal, although new, is already so large.

Among the readers whose studies the editor will bear in mind are these: teachers of psychology in higher institutions of learning; biologists and physiologists; anthropologists who are interested in primitive manifestations of psychological laws; physicians who give special attention to mental and nervous diseases; all others interested in the great progress recently made in so many directions in applying more exact methods to the study of the problems of human feelings, will and thought. The advancement of the science will be constantly kept in view, and the journal will be a record of the progress of investigations.

The journal will consist of three parts.

I. *Original contributions of a scientific character.* These will consist partly of experimental investigations on the functions of

the senses and brain, physiological time, psycho-physic law, images and their association, volition, innervation, etc.; and partly of inductive studies of instinct in animals, pyschogenesis in children, and the large fields of morbid and anthropological psychology, not excluding hypnotism, methods of research which will receive special attention; and lastly, the finer anatomy of the sense-organs and the central nervous system, including the latest technical methods, and embryological, comparative and experimental studies of both neurological structure and function.

II. *Digests and reviews.* An attempt will be made in each number to give a conspectus of the more important current psychological literature, and to review significant books, bad as well as good.

III. *Notes, news, brief mentions,* etc.

While articles of unusual importance in the field of logic, the history of philosophy, practical ethics and education will be welcomed, the main object of the journal will be to record the progress of scientific psychology, for which no organ now exists in English.

Controversy so far as possible will be excluded.

The journal will be published quarterly.

THE VARIATIONS OF THE NORMAL KNEE-JERK, AND THEIR RELATION TO THE ACTIVITY OF THE CENTRAL NERVOUS SYSTEM.

BY WARREN PLYMPTON LOMBARD, M. D.

The research described in this paper was made with the purpose of studying the variations to which the knee-jerk is normally subject. The observations reported are based on experiments which were made with a hammer which struck a blow of known force, and with an apparatus that gave an accurate record of the extent of the resulting knee-jerks. The experiments were all made on the writer, who is a healthy man; they extended over six weeks; they were made at two hundred and thirty-nine different times; and they numbered six thousand, six hundred and thirty-nine. Of these experiments only five thousand, four hundred and seventy-six are reported in the tables accompanying this paper.

These experiments demonstrate not only that the extent of the knee-jerk varies with the force of the blow employed, but that when blows of the same force are used the extent of the knee-jerk varies greatly on different days, at different parts of the same day, and even in experiments which rapidly succeed each other.

The cause for these variations in the extent of the knee-jerk are conditions which affect the vigor of the muscles and nerves involved in the process, and,

to a still greater degree, all influences which vary the activity of the central nervous system as a whole, or of special mechanisms of the spinal cord and brain.

Of the many names which have been given to this phenomenon, viz.: "knee phenomenon," "patellar tendon reflex," "myotatic contraction," "knee-kick," "knee-jerk," the last has commended itself to the writer, because it calls attention to the peculiar suddenness of the movement, and does not imply anything with regard to the nature of the process.

The author takes this opportunity to express his thanks to Prof. H. Newell Martin and Professor G. Stanley Hall, for their valuable advice and their great courtesy. He takes pleasure, also, in acknowledging his indebtedness to his co-worker in this research. All the experiments were made upon the writer by his wife, and their value is greatly enhanced by the accuracy and care with which her work was done.

METHOD OF PRODUCING THE KNEE-JERK, AND THE NATURE OF THE PROCESS.—Place the subject in an easy position, with his knee partly flexed, and his leg freely movable; then strike the middle of the ligament, just below the knee pan, a sudden blow. The kick which results has a jerky character, which is quite peculiar.

If a man sits with one leg crossed over the other, the quadriceps muscle of the leg that is uppermost is slightly stretched by the weight of the suspended leg and foot, and the chain composed of the quadriceps tendon, the patella and the patellar ligament, which connects the quadriceps

muscle with the head of the tibia, is subjected to considerable tension. If, now, the ligamentum patellæ be struck, it will be suddenly depressed into the cavity of the joint beneath it, and a jerk will be transmitted by means of the patella and the quadriceps tendon to the quadriceps muscle. Whether the muscle fibres and the motor nerve fibres lying in the muscle are directly stimulated by the mechanical irritation thus brought to them, whether the end organs of the sensory nerves in the end of the tendon near the muscle, and in the muscle itself, are excited by the effect of the blow, and transmit stimuli to the muscle through the afferent spinal nerves, the centers in the spinal cord and the efferent spinal nerves, or whether both of these processes aid to bring about the muscular contraction, is unknown. We only know that the result of the blow is to cause a sudden contraction of the quadriceps, which jerks the foot forward in the characteristic manner.

All the methods by which the knee-jerk may be obtained, are merely different ways of giving the quadriceps muscle a twitch by bringing a sudden strain upon its tendon.

Nature of the Process.—Whatever view is held with regard to the nature of the process, all admit that it is very dependent upon the condition of the reflex arc, and that the only matter of doubt is whether the influence exerted by the spinal cord has the form of a reflex action and occurs after the blow has been struck, or whether it is a continuous reflex influence which prepares the muscle by increasing its tone, and thus renders it more susceptible to the irritation resulting from the

blow. The argument that the time is too short for a reflex act is inconclusive on account of our lack of knowledge of reflex times in general, and the attempt to prove the existence or non-existence of muscle tonus has thus far proved futile. The fact remains, however, that the existence of the knee-jerk is dependent on the integrity of the reflex arc, and, moreover, that the extent of the knee-jerk is greatly influenced by the irritability of the spinal cord.

Causes for Variations in the Extent of the Knee-Jerk.—It is not the intention of the writer to offer the results recorded in this paper as laws applicable to all men. The influences which determine the extent of the knee-jerk are far too numerous and too subtle to be ascertained by a few thousand experiments on any one man. Although, as has been said, the nature of the knee-jerk is not thoroughly understood, we know it to be an elaborate physiological process, involving the action of many different organs, for both experimentation and clinical experience have disclosed that the normal activity of the quadriceps muscle, of the corresponding afferent and efferent spinal nerves and their roots, and of a certain portion of the cord are necessary to its completeness. Since every condition which influences the action of these different organs must necessarily have its effect upon the extent of the knee-jerk, it is not strange that the phenomenon is subject to many variations. This becomes the more apparent if one considers how many influences are continually modifying the activity of nerve and muscle tissue, and, still more, of the delicate mechanisms of the central nervous

system. Not a few of these changes have their origin in the influence exerted by the different parts of the central nervous system on each other, and there can be but little doubt that the mutual dependence of the cerebro-spinal centers is much greater than has generally been supposed. Indeed, it would almost seem as if the nervous connections were so intimate that a change in the activity of any one of these centers would make itself felt in all the rest, as if, to speak figuratively, there were a balancing of nervous tension throughout the nervous system, so that a change in any one part must be felt in all other parts. Thus, though an increase in pressure, due to a sudden production of nerve force, might, perhaps, encounter less resistance, and so make itself felt chiefly in certain directions, it would produce a slight effect throughout the whole system. The picture represents a condition of things similar to that existing in the circulatory system, where a change of pressure brought about at any part tends to be transmitted to all the rest. Far be it from the writer to offer or support a theory of the action of what we call nerve force. The line of thought has been suggested, however, by the results of his own and similar experiments, which have shown that a strong sensory irritation, a voluntary action, or even an emotion, is sufficient to influence the extent of the knee-jerk. It has long been known that the nervous system binds the many organs of the body into a whole, and that through it the condition of every part is made to have its influence on all the rest, but the closeness of this union has never been illustrated with such startling distinctness as it is in the incessant variations of the knee-jerk.

THE DIAGNOSTIC IMPORTANCE OF THE KNEE-JERK. It is now nine years since Westphal and Erb proclaimed the absence of the knee-jerk in Tabes Dorsalis, and during this time physicians have come to regard the test as a part of the regular routine of physical diagnosis. Nevertheless they have never been quite satisfied with it. In spite of the fact that Berger reported, as a result of the examination of 1409 healthy individuals, that it was absent in only 1.56 per cent., and that Bernhardt stated that he had found it absent in all but two of forty-six cases of Tabes, which he had studied, there have been so many contradictory reports in medical journals, and every practitioner has found so much difficulty in getting satisfactory results in the doubtful cases, that the knee-jerk has been gradually drifting into disfavor. It is probable that a reaction is at hand, for the discoveries of the past four years offer an explanation of many of the apparently inexplicable results, and at the same time greatly extend the usefulness of the symptom.

REËNFORCEMENT OF THE KNEE-JERK.—In 1883 Ernst Jendrassik[1] reported his observation that if the hands were clinched just before the ligamentum patellæ was struck, the resulting knee-jerk was greater than it was when the subject was quiet.

Jendrassik's interesting discovery was made the subject of the most careful study by Dr. S. Weir Mitchell and Dr. Morris J. Lewis,[2] and they were

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[1]Beiträge zur Lehre von den Schnenreflexen.—Deutsches Archiv. f. klin. Med. Bd. 33, s. 177, 1883.

[2]Physiological Studies of the Knee-jerk and of the Reactions of Muscles under Mechanical and other Excitants.—*The Philadelphia Medical News*, Feb. 13 and 20, 1886.

able, not only to corroborate his results, but to show that the knee-jerk was subject to the most extensive variations, even during health, and that these variations probably occurred by means of alterations in the activity of the nerve centres, upon whose integrity the knee-jerk has been found to be dependent. Although the work of these observers is, as far as the writer has been able to test it, correct in every particular, it has been received with a certain amount of scepticism, because their remarkable results are based on observation alone, and not upon any record which can be a proof to others. It is, indeed, wonderful that, trusting as they did to the ability of the hand to deliver a series of blows of constant force, and to the eye to observe slight differences in the extent of the knee-jerk, they should have been able to discover so many facts and to prophesy truly the discovery of so many others. Men who have not their keen power of observation obtain their results with difficulty, and regard them with doubt, and are half inclined to deny all except the results that can be obtained by the coarsest experiments.

THE EXPERIMENTS OF THE AUTHOR.

It seemed to the author that before the knee-jerk could take its proper place as an aid to physical diagnosis, or, still more, as a means of investigating the influences which affect the activity of the central nervous system, there must be devised, first, a method of striking the ligamentum patellæ a blow of known force, and, second, a method of recording the extent of the resulting knee-jerk. If one could be sure of giving the same stimulus throughout a

large series of experiments, and could obtain records of the resulting movements of the leg, one could definitely determine the limits of the normal knee-jerk and the variations which it undergoes under normal conditions. With these thoughts in mind the writer entered upon the research recorded in this paper.

THE APPARATUS EMPLOYED.

1. *A hammer by which it was possible to strike a blow of any desired force.* (See Plate I, Fig. 1.)—Several methods suggested themselves by which one might strike the ligamentum patellæ a blow of any desired force. Of these, the one which upon trial commended itself most highly, was to suspend a hammer by an axis passed through its handle. The hammer could then be made to fall like a pendulum, and would strike a blow, the force of which would depend on the weight of the hammer and on the height from which it fell.

Two instruments of nearly the same construction were employed, the one in the first series of experiments, the other in the second series.[1] The construction of these instruments is shown in Fig. 1, and was in general as follows:

The head of the hammer, *a*, which was made of iron, was 10.5 cm. long, 2.5 cm. wide, and 2.5 cm. thick, and weighed 346.5 gms. It was narrowed at either end to a smooth rounded edge, one of these edges being vertical and the other horizontal.

The handle of the hammer, *b*, a steel rod 22 cm. long and 1 cm. in diameter, weighing 100 gms., passed through a hole which was bored vertically through the middle of the head of the hammer, and protruded a few mm. from the lower side of the head, *c*. The head was fastened to the handle by a screw; and it was so placed that its middle point was just 20 cm. distant from the middle of the axis, which supported the hammer.

The axis, *d*, passed through the handle of the hammer as close as possible to its upper end. It was a steel rod, 5 cm. long and 5 mm. in diameter, and it was pivoted at either end on steel points. The handle was fastened to the axis at about 1 cm. from its inner end.

The screws, *e e*, on which the axis was pivoted, were held by two heavy pieces of brass, *f f*, which extended downward from the horizontal steel rod, *g g*, which supported the whole apparatus, and which was itself clamped at any desired height, on a substantial standard, with a heavy iron base.

A brass plate, *h*, 2.5 mm. thick, was fastened by its upper left hand corner to the supporting rod and to the back of the heavy piece of brass which held the inner pivot, in a plane parallel to that cut by the handle of the hammer when it fell, and with its face

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[1]One of these hammers was made, through the kindness of Prof. H. Newell Martin, by the mechanic at the Biological Laboratory, the other by Lehman, instrument maker, Baltimore.

looking toward the hammer. The left edge of the plate was vertical, the upper edge horizontal, and the right and lower borders formed an arc whose centre would be cut by a line drawn through the pivots supporting the axis of the hammers. A scale of 90° was engraved on this plate a centimeter from its curved edge, and in such a way that 0° corresponded to the middle of the handle of the hammer when it was hanging in the position determined by the force of gravity.

On the back of this plate and parallel to its surface there swung from an axis, whose centre would be cut by a line drawn through the pivots supporting the axis of the hammer, a heavy strip of brass, *i*, 3.5 mm. thick, 25.5 cm. long and 2.5 cm. wide.

This swinging arm bore on its face a small brass plate, *k*, which had a lip which slightly lapped over the curved border of the plate on which the scale was engraved. This small plate was held in place on the arm by two pins and a thumb screw, *l*, which had its head on the back of the arm. When the thumb screw was screwed home it pressed the lip, like a clamp, tightly down on the border of the large plate at any desired place. This clamp bore on the middle of its face an index, *m*, the point of which was directed to the scale engraved on the large plate and determined the position of the arm.

The free end of the arm terminated in a catch, by means of which the hammer could be held and easily be released whenever it was desired. This catch had the following construction viz.: A heavy block of brass, *n*, was fastened to the end of the arm in such a position that the end of the handle of the hammer which protruded beyond the head would just swing clear of its upper surface. A steel spring forced a small steel catch, *o*, up through a hole in the brass block, so that it projected slightly beyond the surface and obstructed the fall of the hammer by catching the handle where it protruded from the head. The lower part of the block of brass was cut away, so as to make room for a lever, *p*, which had the shape of an inverted L, and which was pivoted at the end of its short arm on the lower end of the catch, and again at the place where the two arms of the L meet, to the solid brass block. By means of this lever the catch could be drawn down and the hammer released.

In all the experiments reported in this paper the subject was reclining, with outstretched leg, (see Fig. 2,) and inasmuch as the ligamentum patellæ was horizontal, the blow was struck with the vertical edge of the hammer. In certain other experiments, in which the subject sat with dangling legs, the ligament held a vertical position and the head of the hammer had to be turned around and its horizontal edge used.

2. *The couch and the supports for the thigh and foot.*[1]—See Plate I, Fig. 2, *a*.)—The following arrangements were made, first, to insure the subject an absolutely comfortable position and freedom from all avoidable reënforcing influences; second, to relieve the quadriceps muscle from the weight of the foot, and so permit its slightest contraction to produce a visible movement.

The man experimented upon lay on his left side, upon a comfort-

[1]These arrangements were the same as those employed by the writer in a previous research, viz.: "Is the Knee-Kick a Reflex Act?"—*The Amer. Jour. of Med. Sciences*, Jan., 1887.

able couch, so formed as to support the back and head. (See Fig. 2.) The right thigh rested in a splint of plaster of Paris, shaped so as to conform to inner and posterior surface, and of such a height as to hold the right knee on a level with the hip joint. The right foot was supported at the same height by a swing suspended by a long cord from the ceiling.

3. *The recording apparatus.*—(See Plate I, Fig. 2, *b*.)—The amount of the knee-jerk was revealed in the movement of the foot which it produced, and the extent of this movement was automatically recorded.

A long, light but stiff steel rod extended horizontally backward from the awning on which the foot rested, and at right angles to the lower leg. It was fastened to the back of the swing by a ball and socket-joint and it rested, near its free end, in the groove in the circumference of a wheel, which turned so easily as to rotate under the weight of the rod when the latter was pulled forward or pushed backward by the swinging foot.

A steel needle was fastened on the rod at right angles to it, and wrote with its point on a sheet of glazed paper which had been stretched on a board, blackened by the soot of a gas flame, and placed horizontally at a short distance below the horizontal rod. As the foot was jerked forward by the sudden contraction of the quadriceps muscle, following the blow on the ligament, the needle was dragged across the blackened paper and wrote the extent of the movement. As the muscle relaxed again the foot swung back to its original position, *i. e.*, that which was determined by the balancing of the tension of the antagonistic flexors and extensors of the knee.

The under surface of the board on which the paper was stretched was crossed by two parallel grooves, which corresponded to two glass tracks on the little table on which the board rested. After each experiment the board was made to slide a little to one side, so as to bring a fresh surface of the paper under the needle. The mark made by the needle when the board was thus moved recorded the position of the foot when all was quiet and gave a base line from which to measure the extent of the movements of the foot. At the end of the experiments the records thus obtained were "fixed" by being passed through an alcoholic solution of brown shellac, and the distance moved by the foot as a result of each knee-jerk was measured in mm. and tabulated.

In the experiments in which the effect of respiration on the knee-jerk was studied it was necessary that the record should be made on a moving surface, and therefore the blackened paper was stretched on the drum of a Kymographion.

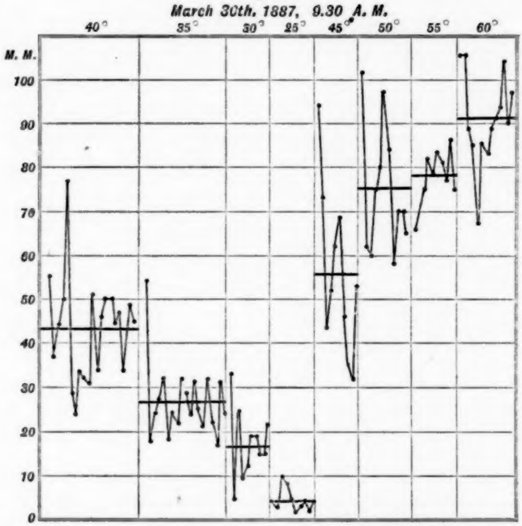
The Experiments.

Effect of successive blows of the same and of different strengths.—The first experiments made with the apparatus described were to determine

how far the extent of the knee-jerk is dependent on the force of the blow. No one who has ever tried to call out the phenomenon can have any doubt that its amount varies with the force of the blow, but in our experiments the closeness of the relation did not at once appear, because we were confronted forthwith by the puzzle which has faced us throughout our work, and which still remains, to a great extent, undeciphered. We found, namely, that if a number of blows of the same force were struck at definite intervals, from exactly the same direction, and on the same part of the ligamentum patellæ, no two of the resulting knee-jerks were of the same extent. Naturally, the sources of reënforcement described by Dr. L. Weir Mitchell and Dr. Morris J. Lewis were looked to for an explanation, but none such could be found. The subject was lying completely at rest in a comfortable position and was conscious of no irritation. His eyes were closed, all the muscles were passive, and the whole body was, as far as possible, in a state of rest. It was then suggested that the force of the blow be increased. This was done, and though similar variations in the extent of the knee-jerk were seen, the movements were found, as a whole, to be greater than before. It was soon ascertained that though blows of the same strength called forth knee-jerks of very different amounts, the averages gained from several groups of twenty or more experiments each, made by striking blows of a certain force, were almost exactly the same, and furthermore, that if the force of the blow was altered, the averages of such groups of experiments made with blows of different strengths, were greater or less, according as the force of the

blow had been increased or decreased. These results will be better understood by reference to Fig. 1.

Fig. 1.



The base line, *o*, shows the position of the recording needle when the leg is quiet, and each of the dots connected by the curve shows, in millimetres, the distance which the foot moved as a result of a blow on the ligamentum patellæ. The experiments which are grouped together show how different may be the extent of the knee-jerks which are obtained by blows which have the same force and which are produced by letting the hammer fall through an arc of the number of degrees given at the top of the table. The heavy horizontal lines which cross such groups indicate the average of the enclosed experiments.

Search for errors in the method employed.—The fact that blows of the same force evoked knee-jerks of very variable extent was, as has been said, an entire surprise to us. The most careful examination failed to reveal any mechanical cause. The hammer fell from exactly the same height and was released in just the same way each time, and as

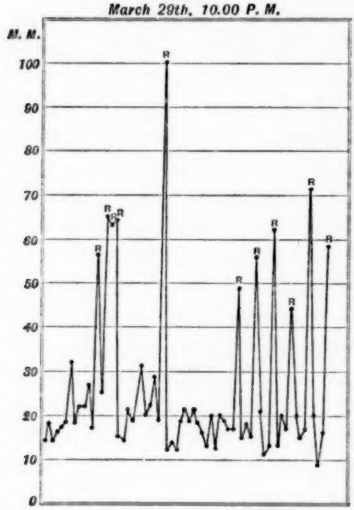
there was no appreciable friction in the apparatus, there could be no doubt that it gave a blow of definite force. The blows were delivered at intervals of fifteen seconds; therefore, the variations could not be due to a wearying of the muscle. Moreover, the knee-jerk was often greater at the end of a series of experiments than at the beginning. The only chance for error seemed to lie in the possibility that the position of the leg was changed slightly from time to time, and that the hammer did not strike the ligament at exactly the same place each time. This question was carefully studied and we were unable to find that there was any such change of position. Moreover, we discovered that it made no appreciable difference in the extent of the knee-jerk whether the hammer struck exactly the middle of the ligament, as we always tried to have it do, or a little above or below that point. Having ruled out all possible sources of error, we were compelled to conclude that the variations which we saw were due to changes which occurred within the individual and which reënforced the action of the mechanisms which produce the knee-jerk. Succeeding experiments proved that there was no lack of reënforcing influences.

The variations seen were compared with strongly reënforced knee-jerks.—Having once assumed that the variations which we had seen were due to some reënforcing influence, we had the curiosity to compare the largest of the knee-jerks, obtained when the subject was entirely quiet, with those which should result from some of the vigorous forms of reënforcement, described by Mitchell and Lewis, such as clinching the hands or clinching the teeth. The results of a few experiments, in

which the reënforcements caused by clinching the teeth were compared with knee-jerks obtained during rest, are shown in Fig. 2. Had a still more active form of reënforcement been employed, probably still greater differences would have been seen. The reënforced knee-jerks, which resulted from voluntarily clinching the teeth, were so extensive as to convince us that the unknown sources of reënforcement, which were continually influencing the knee-jerk, were comparatively weak phenomena.

Fig. 2.

March 29th, 10.00 P. M.



Aim of Experiments of Series I.—It was a great temptation to us to immediately begin to study the effects of different methods of reënforcing the knee-jerk, but we resisted the impulse, knowing that it was much more important to lay a sure foundation for such work by patient and careful study of the extent of the normal knee-jerk when not subject to such exciting influences. We, therefore, determined to make a series of experiments which should last over many days, and which should determine the extent of the knee-jerk in the case of one man who was well, and who was leading his usual regular life. We could not help hoping that in

the course of such experiments many of the more ordinary forms of reënforcement would reveal themselves to us.

Routine of Experiments.—Such a research was accordingly undertaken. The experiments were made on the writer. They extended from April 1st to April 14th inclusive.[1] The condition of the knee-jerk was examined seven times a day, and twenty-five experiments were made at each examination. The hours chosen for the experiments were as follows, viz.: 8.15 A. M., immediately upon rising; 9.15 A. M., soon after breakfast; 1.15 P. M., just before lunch; 2.15 P. M., just after lunch; 6.15 P. M., just before dinner; 8.00 P. M., soon after dinner; and 11 P. M., just before going to bed. For various reasons it was not always possible to make the experiments at exactly the schedule time, but it was seldom that the time of the experiment varied half an hour from that given. The total number of examinations in this series was 93, and the total number of experiments was 2,321. The many experiments which were made at other than the schedule times are not included in these figures.

In the case of each experiment, the hammer was so placed that, when it was hanging free, it just touched the skin over the middle of the ligament. It was then raised through an arc of 40° and allowed to rest on the catch. At the proper moment it was

[1]Throughout the period the subject led a regular life, getting up and going to bed at his usual hours, doing his ordinary work and eating his accustomed fare. It is worth noting that no wine or beer was used during the period, but that a cup of coffee was taken with breakfast and dinner, and a cup of tea with lunch. The subject, as was his habit, smoked one or two cigars a day.

released by a slight movement of the lever, and, inasmuch as it always fell from the same height, it always struck a blow of the same force. The blows were given at intervals of fifteen seconds, and they struck the same part of the ligament each time. For the sake of accuracy all the experiments were made on the bare leg, although examination showed that nearly the same results could be obtained when the knee was covered by a thin layer of clothing. Throughout all the experiments the subject lay with closed eyes, in an absolutely comfortable position, and, as far as was possible, not only avoided all voluntary movements, but directed his thoughts away from the experiment and to some indifferent subject. During the earlier experiments the blows of the hammer were each distinctly felt, but later they were often scarcely noticed, and in many cases the subject went, before the end of the examination, almost, if not quite, asleep.

The following tables give, as far as possible, an accurate account of the experiments and of the condition of the subject at the time that each examination was made. The extent of the movement of the foot resulting from each knee-jerk was accurately measured in millimeters, and tabulated; inasmuch, however, as the reader can be given no correct idea of the subtler influences which governed the extent of each separate knee-jerk, it does not seem profitable to report all these measurements, and only the average of the experiments made at each examination is given. In most cases, indeed, the more delicate influences which determined the extent of the knee-jerk remained undiscovered, but, at times, they unexpectedly revealed themselves, and these discov-

eries give a most interesting and important addition to the physiology of the knee-jerk, and of the central nervous system. These results will be reported by themselves later in the paper.

Explanation of the Tables.—Each table is made in three parts; the first, headed Knee-Jerk, contains the results of the experiments; the second, headed Extracts from Journal, gives, in brief, the way in which the subject spent the day, and, therefore, an idea of his condition at the time of the examination; and the third part, headed U. S. A. Weather Observations, reports the condition of the weather in the morning, afternoon and evening. In the first column of the first part of the table, the time at which the examinations were made are set down; in the second column, the number of experiments made at each time is reported; in the third is recorded in millimetres the average extent of the knee-jerk as determined by these experiments; in the fourth are shown the least and greatest knee-jerks got in the examination, and in the fifth is stated, in the number of degrees through which the hammer fell, the least blow by which a recognizable knee-jerk could be obtained. At the bottom of column two the total number of the experiments made during the day is given; next to this, under column three, is written the average knee-jerk for the day, as determined by the total number of experiments; by the side of this are noted the extreme variations of the knee-jerk obtained on this day with the blow of standard force, *i. e.*, when the hammer fell through an arc of 40°; and, finally, under column five, one is told what was the least blow which was capable, at any time during the day, of producing a visible knee-jerk.

No. 1, Series I—April 1st, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well and Vigorous.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.30 a.m.	25	36	20–48	27°	Just out of bed and half asleep.	7 a.m.	30.150	31°	84	n.	lt. snow
9.45 "	18	88	55–120	20°	Just after breakfast.						
1.15 p.m.	12	111	90–130	20°	Morning spent writing.						
2.15 "	20	68	29–93	23°	Just after lunch.	3 p.m.	30.089	36°	87	n.e.	lt. snow
6.15 "	23	49	10–78	26°	Afternoon spent writing, head tired.						
8.15 "	26	44	16–75	29°	Just after dinner.						
10.30 "	25	45	22–60	30°	Evening spent reading and writing.	10 p.m.	30.036	34°	88	n.	lt. snow
	149	63	10–130	25°		mean.	30.092	34°			

No. 2, Series I—April 2d, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well, but somewhat fatigued by yesterday's work.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.15 a.m.	26	28	12–42	29°	Just out of bed—sleep again (?)	7 a.m.	29.907	34°	89	n.w.	lt. snow
9.45 "	24	72	45–93	26°	After breakfast.						
1.15 p.m.	23	63	31–94	24°	Morning spent writing.						
2.30 "	27	52	26–75	28°	Just after lunch.	3 p.m.	29.789	52°	38	n.w.	clear.
6.15 "	22	23	7–50	35°	Afternoon spent writing—walk an hour.						
8.15 "	27	33	9–62	30°	Just after dinner.						
10.30 "	20	58	37–91	30°	Ev'g spent reading German with friends	10 p.m.	29.941	46°	38	n.	clear.
	169	47	7–94	29°		mean.	29.879	44°			

No. 3, Series I—April 3d (Sunday), 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well, but feel lazy.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
9.30 a.m.	25	40	33–52	25°	Just out of bed; been awake an hour.	7 a.m.	30.093	43°	55	n.w.	clear.
10.15 "	24	64	45–80	26°	Just after breakfast.						
2.15 p.m.	27	39	21–65	28°	Morning spent writing and reading; no hard work done.						
3.30 "	25	74	39–103	25°	Just after dinner.	3 p.m.	29.992	64°	34	s.w.	clear.
6.15 "	27	33	9–61	30°	Immediately upon return from stroll of two hours.						
7.30 "	24	57	27–88	25°	Just after tea.						
10.45 "	26	18	8–30	35°	Evening spent writing and talking.	10 p.m.	29.980	49°	66	w.	clear.
	178	47	8–103	28°		mean.	30.022	52°			

No. 4, Series I—April 4th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	More rested than yesterday. An enervating day.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.15 a.m.	25	31	7–49	32°	Just out of bed and very sleepy.	7 a.m.	29.928	48°	74	s.	clear.
9.15 "	25	73	43–100	26°	Just after breakfast.						
1.15 p.m.	25	20	6–39	34°	On my feet somewhat—read and talk.						
2.15 "	25	24	7–48	30°	Just after lunch.	3 p.m.	29.724	76°	28	s.w.	clear.
6.15 "	25	27	11–44	36°	Afternoon at laboratory.						
9.20 "	27	21	10–42	37°	An hour after dinner.						
11.00 "	25	22	10–31	37°	A quiet and restful evening.	10 p.m.	29.771	58°	61	n.w.	cloudy
	177	31	6–100	33°		mean.	29.808	61°			

No. 5, Series I—April 5th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well, but not very energetic.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.15 a.m.	21	19	10–30	35°	Just out of bed and very sleepy.	7 a.m.	29.908	45°	41	n.w.	fair.
9.15 "	25	51	42–60	30°	Soon after breakfast.						
1.30 p.m.	13	27	14–47	32°	Morning spent writing.						
2.30 "	25	43	21–75	27°	Soon after lunch.	3 p.m.	30.019	38°	59	n.w.	cloudy.
6.30 "					Wrote till five, then walked an hour.						
7.45 "	24	57	4–82	29°	Just after dinner.						
10.30 "	27	23	12–41	35°	Walk and make a call; read aloud.	10 p.m.	30.187	31°	50	n.w.	clear.
	135	37	4–82	31°		mean.	30.038	38°			

No. 6, Series I—April 6th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimeters.	Extremes.	Lightest Effective Blow.	Well.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.15 a.m.	25	23	12–38	31°	Just out of bed.	7 a.m.	29–332	32°	61	n.w.	fair.
8.30 "	25	51	31–72	27°	Before breakfast and after bath.						
9.30 "	24	79	56–105	25°	Just after breakfast.						
1.15 p.m.	25	49	20–70	27°	Morning spent in making a call and writing.						
2.30 "	26	54	25–82	26°	Just after lunch.	3 p.m.	30–300	48°	26	n. w.	clear.
6.15 "	25	15	7–31	37°	Afternoon spent standing and talking.						
8.00 "	24	32	12–54	30°	Just after dinner.						
11.00 "	24	29	18–55	37°	Evening spent in reading German with friends. At 10.30 the music experiment.	10 p.m.	30.352	41°	39	s.	clear.
	198	47	7–105	34°		mean.	30.328	40°			

No. 7, Series I—April 7th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimeters.	Extremes.	Lightest Effective Blow.		Time.	Barometer.	Thermometer.	Relative Humidity	Wind.	Weather.
					In morning feel well and vigorous, but become nervous by noon, so that I start easily at noises.						
8.15 a.m.	26	29	18–38	36°	Just out of bed.	7 a.m.	29.437	38°	60	n.	cloudy
9.30 a.m.	25	71	45–96	22°	After breakfast and an earnest talk.						
1.15 p.m.	24	66	39–92	22°	Morning spent writing—head dizzy.						
2.15 "	25	34	9–57	25°	Just after lunch.	3 p.m.	30.442	50°	34	e.	cloudy
6.30 "	25	31	10–75	30°	Afternoon spent writing; walk half an hour; head and eyes tired.						
8.00 "	25	52	32–72	29°	After dinner and an earnest talk.						
11.00 "	25	32	18–58	32°	Write till ten, then read aloud an hour.	10 p.m.	30.552	45°	60	e.	cloudy
	175	45	9–96	28°		mean.	30.477	44°			

No. 8, Series I—April 8th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well, slightly tired.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.45 a.m.	25	22	7–40	37°	Just out of bed.	7 a.m.	29.680	41°	66	n.e.	cloudy
9.45 "	25	70	53–87	27°	Just after breakfast.						
1.15 p.m.	28	29	5–87	31°	Wrote a short time; took a walk.						
2.15 "	25	42	17–71	27°	Just after lunch.	3 p.m.	30.566	54°	41	s.e.	clear.
6.15 "	25	44	29–74	22°	Write, call, write.						
8.30 "	28	51	30–72	25°	Just after dinner.						
11.30 "	27	43	22–68	30°	A quiet evening.	10 p.m.	30.536	45°	55	s.	clear.
	183	43	5–87	28°		mean.	30.577	46°			

No. 9, Series I—April 9th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lighest Effective Blow.	Well and vigorous.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.00 a.m.	26	35	15–50	28°	Just out of bed.	7 a.m.	30.525	39°	80	s.	clear.
9.00 "	25	71	48–100	26°	Just after breakfast.						
1.15 p.m.	26	37	11–72	27°	A busy morning; head and back ache.						
2.15 "	25	36	13–64	26°	Just after lunch.	3 p.m.	30.348	61°	54	s.e.	clear.
6.15 "	25	21	8–49	35°	Afternoon spent writing; walk an hour.						
8.15 "	26	33	15–68	31°	Just after dinner.						
10.30 "	25	14	7–27		A walk; read; evening is close.	10 p.m.	30.276	51°	61	s.	clear.
	178	35	7–100	29°		mean.	30.383	50°			

No. 10, Series I—Sunday, April 10th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.15 a.m.	25	13	6–31	39°	Just out of bed.	7 a.m.	30.212	45°	59	s.	clear.
9.30 "	25	53	30–74	31°	Soon after breakfast.						
1.30 p.m.	25	26	5–71	33°	Church; a short walk.						
3.30 "	26	44	17–76		Soon after dinner.	3 p.m.	30.093	83°	25	n.w.	clear.
7.45 "	16	31	18–52		After a walk of two hours.						
11 "	27	17	6–39	37°	After a quiet evening.	10 p.m.	30.077	65°	48		clear.
	144	31	5–76	35°		mean.	30.127	67°			

No. 11, Series I—April 11th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well; seminal emission early this morning.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.15 a.m.	25	9	4–27	39°	Just out of bed.	7 a.m.	30.108	61°	49	w.	clear.
9.30 "	25	40	12–56	30°	Soon after breakfast.						
1.15 p.m.	25	23	5–61	35°	Morning spent writing and talking.						
2.30 "	25	41	21–69	30°	Just after lunch.	3 p.m.	30.024	83°	23	w.	clear.
6.15 "	25	27	0–50	33°	Afternoon spent writing.						
8.00 "	26	25	10–57	35°	Just after dinner.						
11.15 "	25	20	9–32	36°	A walk; listen to reading.	10 p.m.	30.097	72°	34	n.w.	clear.
	176	27	0–69	34°		mean.	30.076	72°			

No. 12, Series I—April 12th, 1887.

Knee-Jerk.					Extracts from Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.30 a.m.	27	20	3–42	37°	Just out of bed and very sleepy.	7 a.m.	30.273	58°	67	n.e.	clear.
9.30 "	25	57	38–78	27°	Just after breakfast.						
1.15 p.m.	26	21	9–41	33°	Morning spent standing and walking.						
2.30 "	21	42	25–60	29°	Soon after lunch.	3 p.m.	30.216	65°	52	s.e.	clear.
6.15 "	25	29	6–65	33°	Afternoon at laboratory. On my feet much of the time.						
8 "	25	34	16–71	30°	Soon after dinner.						
12 "	16	10	3–24	33°	Evening with friends. Walk a mile.	10 p.m.	30.253	51°	68	e.	fair.
	165	30	3–78	32°		mean.	30.247	58°			

No. 13, Series I—April 13th, 1887.

Knee-Jerk.					Extracts From Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest Effective Blow.	Well.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.45 a.m.	24	25	11–50	31°	Just out of bed.	7 a.m.	30.295	47°	89	e.	cloudy.
9.30 "	15	74	57–106	22°	Soon after breakfast.						
1.00 p.m.	24	44	25–71	26°	A busy morning.						
2.30 "	25	46	24–69		Soon after lunch.	3 p.m.	30.266	58°	59	s.e.	clear.
6.30 "	25	42	13–74	25°	Afternoon spent writing.						
8.00 "	25	50	27–76	25°	Just after dinner.						
11.00 "	29	20	3–41	33°	Evening spent with friends.	10 p.m.	30.351	46°	64	s.e.	fair.
	167	43	3–106	27°		mean.	30.304	41°			

No. 14, Series I—April 14th, 1887.

Knee-Jerk.					Extracts from Journal.	U. S. A. Weather Observations.					
Time of Examination.	No. of Experiments.	Average Movement in Millimetres.	Extremes.	Lightest effective Blow.	Well.	Time.	Barometer.	Thermometer.	Relative Humidity.	Wind.	Weather.
8.30 a.m.	27	20	7–42	35°	Just out of bed.	7 a.m.	30.317	44°	67	s.e.	fair.
9.15 "	25	53	33–79	20°	Just after breakfast.						
1.15 p.m.	25	42	14–64	24°	Morning spent writing.						
1.45 "	25	41	26–64	25°	After lunch.	3 p.m.	30.184	56°	43	s.e.	clear.
6.30 "	24	16	3–33	34°	Wrote till five; walked an hour.						
8.15 "	25	36	16–64	30°	Just after dinner.	10 p.m.	30.140	49°	65	s.e.	clear.
	151	35	3–79	28°		mean.	30.216	50°			

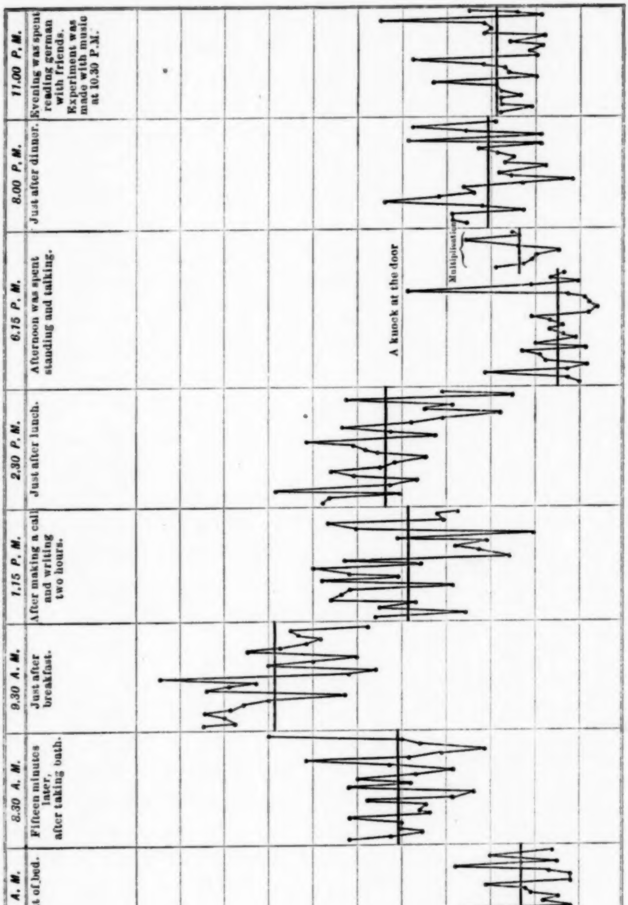
Common experience teaches that when one is well, there are three principal influences which lower the activity of the body: fatigue, hunger and depressing weather; while rest, a meal and invigorating weather increase the activity. One has also learned that even when the general condition is depressed by these influences it may be temporarily roused by any cause of mental excitement, and that when it is in a vigorous state it may be temporarily lowered by drowsiness.

Substitute in the above statements knee-jerk for activity of the body, and they will be equally true. These facts were illustrated in our experiments by a diurnal decline of the knee-jerk, interrupted at meal time, and varied by changes in the weather, fatigue, and by causes of mental excitement.

Explanation of the chart, Fig. 3, *which shows all the variations of the knee-jerk which occurred in the course of one day of this series of experiments.*— Before studying the results of the experiments as a whole, the writer wishes to illustrate still more clearly the great number of variations to which the knee-jerk is subject in the course of a single day.

The following chart shows the extent of the movement of the foot in millimetres in each experiment taken in the course of one day. All the experiments made at the time of one examination are grouped together under the figures which show the time at which the examination was made. Each dot represents a separate knee-jerk, and the connecting lines are given to enable the eye to more readily grasp the extent of the variations. The heavy horizontal lines show the average of all the experiments through which they are drawn. At the top of the table is given roughly the day's journal, and in the body of the table are remarks accounting for reënforcements, the causes of which were thought to have been recognized.

Fig. 3.


April 6th 1887
M. M.
8.15 A. M.
Just out of bed.
8.30 A. M.
Fifteen minutes later, after taking bath.
9.30 A. M.
Just after breakfast.
1.15 P. M.
After making a call and writing two hours.
2.30 P. M.
Just after lunch.
6.15 P. M.
Afternoon was spent standing and talking.
A knock at the door
Multiplication
8.00 P. M.
Just after dinner.
11.00 P. M.
Evening was spent reading german with friends. Experiment was made with music at 10.30 P.M.
110
100
90
80
70
60
50
40
30
20
10
0

Study of the Chart.—At a glance one sees that, at whatever time the examination was made, the extent of the knee-jerk varied greatly in succeeding experiments. He also notices that the average of the experiments made when the subject was just out of bed, and not thoroughly roused, was low; that in the examination made fifteen minutes later, after the bath had been taken, it was higher; and that an hour later, immediately after breakfast, it was still higher. From this time on, however, the knee-jerk declined, being considerably lower before lunch, and though slightly higher just after lunch, being very much lower just before dinner. After dinner it recovered somewhat, but only to fall again, if but slightly, and at bed time it was very much less than it was just after breakfast, and even less than it was just after the bath taken before breakfast.

To judge from this one day, then, there is a great difference in the extent of the knee-jerk, even in succeeding experiments, and a still greater difference between experiments taken at different times in the day, the knee-jerk being greatest immediately after breakfast, and, in spite of the fact that each meal tends to increase it, being much lower at bed time.

The discussion of the reënforcements which were observed during this day will be deferred until later in the paper.

Diurnal Variation of the Knee-Jerk.—Is the diurnal variation of the knee-jerk seen on April 6th a constant phenomenon? This question is answered by the following table of the averages, compiled from all the experiments which were taken in this series:

Explanation of the Table.—In the first column of the table is given the date of the experiments, and in the first line the hours of the day at which the examinations were made. Beneath the hours, on the same line with the dates, is arranged the average of all the experiments made in the seven examinations of the corresponding day. The table, therefore, enables one readily to compare the results of all the experiments made on each day and of all the experiments made at the same hour on different days. At the bottom of the table, under the hours, is given the average of all the experiments taken at the same hour on all the different days of the series. In addition to this, the table shows the average extent of the knee-jerk for each day; the number of examinations and experiments which were made on each day; and the mean of the barometer and thermometer for each day. Finally, at the bottom of the table, beneath these columns, is placed the average knee-jerk, as determined by all the experiments in the series, and the mean barometer and thermometer for the two weeks under consideration.

SUMMARY OF RESULTS OF EXAMINATIONS OF SERIES I.

April, 1887.	8-9	9-10	1-2	2-3	6-7	8-9	10-11	Average K. J. in mm.	Total No. of Examinations.	Total No. of Experiments.	Mean Barometer.	Mean Thermometer.
1st..........	36	88	111	68	49	44	45	63	7	149	30.092	34
2d..........	28	72	63	52	23	33	58	47	7	169	29.879	44
3d (Sunday)	40*	64*	39	74*	33	57**	18	47	7	178	30.022	52
4th..........	31	73	20	24	27	21	22	31	7	177	29.808	61
5th..........	19	51	27	43	..	57	23	37	6	135	30.038	38
6th.........	23	79	49	54	15	32	29	40	7	173	30.328	40
7th..........	29	71	66	34	31	52	32	45	7	175	30.477	44
8th..........	22	70	29	42	44	51	43	43	7	183	30.577	46
9th..........	35	71	37	36	21	33	14	35	7	178	30.383	50
10 (Sunday)	13	53	26	44*	..	31	17	31	6	144	30.127	67
11th.........	9	40	23	41	27	25	20	27	7	176	30.076	72
12th.........	20	57	21	42	29	34	10	30	7	165	30.247	58
13th.........	25	74	44	46	42	50	20	43	7	167	30.304	51
14th.........	20	53	42	41	16	36	..	35	6	151	30.216	50
	25	65	43	47	30	40	27	40	95	2320	30.184†	50°.5‡

*The examination was one hour late.
**The examination was one hour early.
†Mean barometer for April, for 16 years, was 29.995.
‡Mean thermometer for April, for 16 years, was 53.°3.

A little study of the table tells one that the lowest averages were obtained in examinations taken at the beginning and end of each day. Inasmuch, however, as the first examination was made when the subject was just out of bed and still half asleep, while all the rest were made after he had been thoroughly roused, it would seem that the first examination, though of great interest, could scarcely be compared to the rest. Of the six remaining examinations, the one taken immediately after breakfast has, usually, decidedly the largest average. There are exceptions to this rule, however; thus, on April 1st the highest average was got at 1.15 P. M.; on April 3d at 3.30 P. M ; on April 5th at 7.45 P.M; and on April 11th at 2.30 P. M. No cause suggests itself why an exception to the rule should have occurred on April 1st, unless, indeed, some unusual excitement prevailed at the time of the examination. The same may be said of April 3d; this day was Sunday, however, and the change of hours and the absence of hard work in the morning may have influenced the result. On the 5th of April, the disturbing cause was without doubt the weather. The barometer, which was low in the morning, as can be seen by referring to the table of the day, rose, and the temperature fell as the day advanced. Later in the paper it will be shown that such changes are potent influences and always tend to increase the extent of the knee-jerk. With regard to April 11th, it can only be said that the journal reports that there was a seminal emission early in the morning. Whether this fact accounts for the depression of the averages early in the day cannot be definitely decided. In spite of the exceptions noted, it is just to

say that the knee-jerk is generally highest in the early part of the day. This conclusion corresponds with the feeling of the subject, who is usually most vigorous in the early part of the day, but who occasionally does not feel like active work until considerably later.

The last line of the table contains the averages derived from all the experiments taken at each of the regular examinations, arranged according to the hours at which the examinations were made. These averages corroborate what has been already stated, that there is a diurnal variation of the knee-jerk, that it is greatest in the morning, just after the first meal, and that it is lower at night. This falling off of the knee-jerk can be scarcely attributed to anything except a depression of the condition of the body as a whole dependent on weariness, and, as far as the writer can judge, it is proportional to the degree of fatigue, except when counteracted by some reënforcing influences. Although the knee-jerk tends to become less as the day goes on, one sees in the averages given at the bottom of the table, viz.: 25, 65, 43, 47, 30, 40, 27, that the decline is an interrupted one, and this brings us to the consideration of the effect of hunger.

THE EFFECT OF MEALS ON THE KNEE-JERK.—It may be stated, as a rule, that the knee-jerk is higher after each meal than before it. This rule, however, like every other, has its exceptions, and they are shown in the following table :

Explanation of the Table.—In this table the average knee-jerk, before and after each meal, is given, and in the columns following the difference between

these averages is placed under the sign +, if the knee-jerk was greater after the meal, and under the sign —, if it was greater before the meal.

Effect of Meals Upon the Knee-Jerk.

Date.	Breakfast.				Lunch.				Dinner.			
April, 1887.	Before.	After.	+	−	Before.	After.	+	−	Before.	After.	+	−
1st........	36	88	52	....	111	68	..	43	49	44	..	5
2d..........	28	72	44	....	63	52	..	11	23	33	10	....
3d (Sunday)	40	64	24	....	36	74	38	....	33	57	24	....
4th.........	31	73	42	....	20	24	4	....	27	21	..	6
5th.........	19	51	32	....	27	43	16	....	..	57	..	....
6th.........	23	79	56	....	49	54	5	....	15	32	17	....
7th.........	29	71	42	....	66	34	..	32	31	52	21	....
8th.........	22	70	48	....	29	42	13	....	44	51	7	...
9th.........	35	71	36	....	37	36	..	1	21	33	12	....
10 (Sunday)	13	53	40	....	26	44	18	....	..	31	..	....
11th........	9	40	31	....	23	41	18	....	27	25	..	2
12th........	20	57	37	....	21	42	21	....	29	34	5	....
13th........	25	74	49	....	44	46	2	....	42	50	8	....
14th.... ...	20	53	33	....	42	41	..	1	16	36	20	....
	25	65	40		43	47	4		30	40	10	

From the table, one learns that the knee-jerk was always greater after, than before, breakfast. As has been said, however, this comparison is scarcely just, because the subject was not fully awake at the time of the first examination. One also sees that the average was greater after than before lunch, on nine of the fourteen days studied; that on two more days, the 9th and 14th, there was only the dif-

ference of one mm. between the averages of the two examinations, and that on three days, the 1st, 2d and 7th, the average was considerably greater before than after lunch. With regard to the effect of dinner, one observes that the average was greater after than before dinner on nine of the twelve days on which both examinations were made, that it was only two mm. greater before than after dinner on one of the remaining days, the 11th, and that it was 5 mm. and 6 mm. greater before than after dinner on, respectively, the 1st and the 4th.

As everyone knows, the result of a hearty meal is to make one feel quiet and indisposed to work, while the effect of a moderate meal is to rest and invigorate. If one has been working hard up to the moment of meal time, the tire is at first unnoticed, because the excitement still remains, and it is only after an interval of quiet that one becomes conscious of the weariness. Inasmuch as the activity of the mind has a great influence upon the extent of the knee-jerk, as will be shown hereafter, it is probable that the mental condition is in a great degree responsible for the exceptions which have been noted. An examination of the averages derived from all the experiments taken during the two weeks, before and after the three meals, is to be found at the bottom of the table, and it shows that the knee-jerk was, on the average, always greater after, than before, each of the three meals. It may be justly stated, therefore, that the effect of a meal is to increase the knee-jerk, but that this tendency is not so strong but that it is frequently overcome by counteracting influences.

It may be well to note here that no wine or beer

was used with the meals, but that coffee was taken with breakfast and dinner, and tea with lunch.

EFFECT OF MUSCULAR FATIGUE UPON THE KNEE-JERK.—As has been shown, the knee-jerk, by its diurnal variations, illustrates the gradual loss of vigor which the body, as a whole, suffers from morning till bed time, and the temporary and partial recoveries which it undergoes, as a result of the fresh supplies of nutriment and of rest which it obtains at each meal.

The phenomenon is still more markedly affected by the voluntary exercise of the muscles which are directly concerned in its production. A proof of this statement is offered in the experiments recorded in the following table :

Time of Exam.	Extracts from Journal.	Average Knee-Jerk.
11 A. M.	After writing half hour	71 mm.
11.15 A. M.	After walking up and down stairs 15 min.	28 mm.
11.45 A. M.	After talking earnestly	32 mm.
1 P. M.	After studying curves an hour	44 mm.
2.15 P. M.	Just after lunch	46 mm.

Here one sees that the effect of walking up and down stairs for fifteen minutes was to decrease the average extent of the knee-jerk from 71 mm. to 28 mm. There can be no doubt but that the change was the result of the exercise, for during the next two hours of quiet the average gradually increased, in spite of the fact that hunger and general fatigue must have tended to lower it. Numerous illustrations of the decrease of the knee-jerk, as a result of the voluntary exercise of the muscles of the leg, have occurred in the course of our experiments; thus, we have always found that the phenomenon was markedly decreased by a walk or even a short stroll. This observation is of importance to the

practicing physician, because it teaches him not to expect a vigorous knee-jerk from a patient who has walked a mile to his office.

How far the lessening of the movement seen in such cases is due to fatigue of the muscles which extend the knee, and how far it is dependent on fatigue of the central nervous mechanisms, is a problem, the solution of which would require a special research, which we have as yet had no time to undertake. That the extent of the knee-jerk is intimately dependent on the activity of the spinal centers cannot be doubted, and this dependence probably accounts to a great extent for the diurnal variations which we have called attention to, but it is not at all clear that it is the wearying of the spinal centers which accounts for the low knee-jerk which is found to result from a walk.

Effect of Mental Fatigue.—In our experiments we find that the brain exerts an indirect, but nevertheless very considerable, influence over the extent of the knee-jerk, as will be shown when we come to study the subject of reënforcements. It is rarely, if ever, that the mechanisms of the brain act singly, and consequently it is most difficult to trace the reënforcing influences to their proper source. Apparently, however, it is those centers which are the seat of the will, and of the emotions, rather than those by which we perform such forms of mental work as adding, memorizing and planning, that are chiefly concerned in reënforcing the knee-jerk. In our experiments we have not found that short periods of mental work have any effect on the extent of the knee-jerk, and when the work extended

over long intervals the effects of hunger and of general fatigue disguised the results.

Unusual Mental Fatigue.—Twice in the course of the experiments the subject spent too many hours in measuring and tabulating results, and the work, together with the depressing weather which prevailed at the time, caused unusual mental fatigue. The weariness showed itself in a slight dizziness and an irritability which made him start at unexpected noises. During the experiments which were made at this time the peculiar sensation in the muscle, resulting from the jerk produced by the blow, or from the sudden contraction of the muscle, a feeling which was ordinarily unnoticed became so acute and so disagreeable that toward the end of the examination it was hard for the subject to lie quietly. He had a strong desire to contract the muscles of the limb and foot of the side experimented upon, the feeling being comparable to that which one has in the muscles of the jaw after biting a piece of rubber hard. The more one thought of it the stronger became the temptation to move, until it seemed to the subject as if he were keeping quiet by a positive act of the will. This nervous desire to contract many muscles of the limb was suggestive of a central rather than a peripheral excitability, and at first thought was referred to the spinal cord. The idea suggested itself that the brain was weary and was therefore unable to exert the inhibiting influence which many suppose it to have over the centres of the cord, and that these centres being partially freed from control, were unusually active. The subject found, however, that by directing his thoughts away from the experiments and to other subjects, by compelling himself to give

his whole attention to planning an apparatus, for instance, he could, after a little time, forget the irritating sensation. When the thoughts were thus engaged on other matters, it would seem that the spinal cord would be more free from cerebral control than when the mind was wholly interested in the knee-jerk, and yet the disagreeable sensation and the exaggerated movements ceased, which proved the excitability to be in the brain rather than in the cord. It was never found during this research that it was possible to inhibit the extent of the knee-jerk by an act of the will, but the subject noticed again and again that when the knee-jerk was being reënforced by unusual cerebral activity, especially if of an emotional character, the extent of the movement could be reduced by directing the thoughts to some indifferent subject, for instance, by quietly concentrating the attention on the warmth of the skin of the hand.

As far as the writer can judge, from his experiments, fatigue, whether bodily or mental, is accompanied by a decrease of the knee-jerk, and the exceptions recorded above, when excessive mental weariness was found to increase the extent of the phenomenon, was due to the fact that the mind was in an irritable condition, and reënforced the knee-jerk. This matter will become clearer after a review of the ways in which the knee-jerk can be reënforced.

[Since the above was written the attention of the author has been called to a short article by Maximilian Sternberg, in the Centralblatt für Physiologie, May, 1887, in which the writer relates his experiments, and states his conclusion that an increase of the tendon reflex is a sign of general fatigue, whether produced by long-continued physical or mental exertion, and explains the fact as possibly resulting from the withdrawal of cerebral inhibition. This result is the opposite of that reached by the author of this

paper. The apparent contradiction, however, may be explained by the fact that Sternberg's experiments dealt with cases of extreme fatigue, while those of the author were confined to a study of an amount of fatigue such as would ordinarily occur in the course of a day. The whole subject of the effect of different kinds and of different degrees of fatigue on the knee-jerk, is worthy of further study.]

REËNFORCEMENTS OF THE KNEE-JERK.—As has been said, successive blows of the same force, delivered at like intervals, and on exactly the same part of the ligamentum patellæ, called forth knee-jerks of different strengths. Since the stimulus was the same in each case, the causes of the variations must be sought within the individual. It immediately suggests itself, that it is possible that the irritability of the muscle is continually undergoing change. When one, however, considers how equally a muscle which has been separated from the influence of the central nervous system, by division of its nerve, responds to like stimuli, he is forced to admit that the variations in the knee-jerk must result from changes originating outside of the muscle, and, most probably, in the central mechanisms with which it is connected. If the knee-jerk be a reflex act, as many suppose, its variations may well be due to alterations in the activity of the reflex centers of the cord; if it be a peripheral act, it may be that the variations are dependent on changes in the tension of the muscle, resulting from changes of activity of the centers of the spinal cord, which are thought to control its tonus. In fact, whatever the nature of the process resulting in the knee-jerk, one must look to the centers of the spinal cord as the source of the variations which have been noticed. What are the influences which determine the activity of these centers? It is wisest

to not try to answer this question, and to attack the subject from another side.

As has been said, it was not the object of our research to determine the causes of the reënforcement of the knee-jerk, but we soon found that we could not study the subject at all without taking this question into consideration. It is not too much to say, that every knee-jerk which one obtains, is the resultant of a vast number of reënforcing influences, which are for the most part unrecognizable, but which occasionally reveal themselves, though singly, when some source of reënforcement is so active as to attract attention.

REËNFORCEMENT CAUSED BY IRRITATION OF THE SKIN.—For instance, a sensory irritation, such as a prickling or itching of the skin, causes a marked reënforcement. Thus, at the examination at 6.30 P. M., April 5th, the average knee-jerk was 14 mm., and the reënforcement which resulted from a blow which chanced to be given at the moment when the ear itched was 63 mm. (Fig. 4.) Again, at the examination at 12 P. M., on April 12th, the average knee-jerk was 13 mm., and itching of the skin caused a group of reënforcements, viz: 37, 14, 25, 25, (Fig. 5.) With regard to the extent of the reënforcements, one must remember that even when an irritant is con-

Fig. 4.

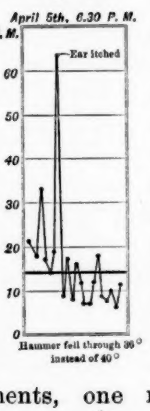
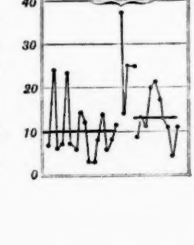


Fig. 5.



tinuously applied, we recognize the sensation, not as constant, but as of varying intensity, and that Mitchell and Lewis found that the extent of the reënforcement depended upon the moment at which the blow was delivered. If the blow falls at the moment that the reënforcing influence is at its height, the resulting movement is more marked than if the knee-jerk is called out a little earlier, or a little later. Thus, in the second example given, the skin was itching all the time, but the intensity of the sensation was much greater at one moment than at another, and the reënforced knee-jerks show a similar difference. The above examples illustrate a fact which was demonstrated many times in the course of our experiments. It was noticed, again and again, that not only such a positive source of irritation to the skin, but anything causing discomfort, as, for instance, a crease in the clothing, or an uncomfortable position, was sufficient to increase the extent of the knee-jerk. These observations corroborate the results of Mitchell and Lewis, who found that painful impressions brought to the skin, as heat, cold, the electric wire brush, etc., were capable of reënforcing the knee-jerk.

Reënforcements Produced by Voluntary Actions. Mitchell and Lewis also found that any voluntary movement, however slight, tended to reënforce the knee-jerk, and in our experiments we saw this fact illustrated over and over again. Thus at the examination at 6.15 P. M., on April 11th, the average knee-jerk was 27 mm., and the movement which resulted from a blow which chanced to fall at the moment the subject was swallowing, was 50 mm.

Fig. 6.
APRIL 11TH 6.15 P. M.

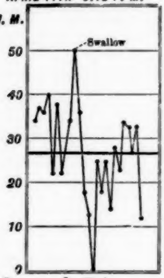
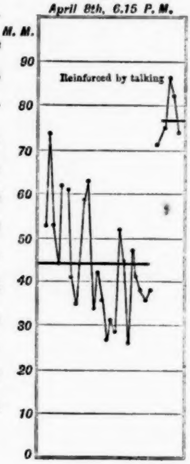


Fig. 7.
April 8th, 6.15 P. M.



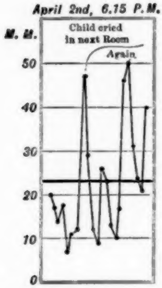
(Fig. 6.) Again, at the examination at 6.15 P. M., on April 8th, the average knee-jerk was 44 mm., and the knee-jerks which were called out immediately after the regular experiments and which were reënforced by talking, measured 71, 75, 86, 82 and 74 mm. (Fig. 7.) As has been said, to get the full effect of the reënforcement, the blow must be delivered at just the right moment after the reënforcing act. When this was done, such active reënforcing acts, as clenching the hands or teeth, enormously increased the movement. (See fig. 4.)

Reënforcements Produced by Exciting the Attention.—All these reënforcing influences were of interest to us chiefly because of our wish to avoid them, and our desire to see blows of the same force call forth knee-jerks of the same extent. When the subject was lying entirely quiet, with closed eyes, in what he felt to be an absolutely comfortable position, the knee-jerks continued to be of variable extent. A cause for some of these variations was, however, soon discovered. During the examination at 6.15 P. M., April 2d, a child in the next room began to cry, but was immediately quieted; in a few moments the child began to cry again and was again quickly quieted. The average

knee-jerk at this examination was 23 mm., and the movement which occurred while the child was crying were 47 mm. and 46 mm. (Fig. 8.) The subject of the experiments was in no way interested in the child and was not conscious of making the slightest movement while it was crying.

Fig. 8.

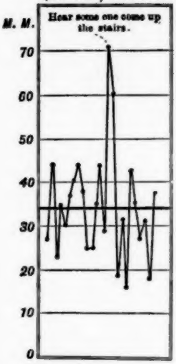
April 2nd, 6.15 P.M.



Three explanations of the reënforcement suggested themselves: One, that the subject had, without knowing it, made a voluntary movement; another, that the sound had acted like other forms of sensory stimulation, which have been found to reënforce, and, finally, that it was possible that the cerebral processes, which accompany the turning of the attention into new channels had, in some way, influenced the action of the distant centres in the cord which control the extent of the knee-jerk.

When the attention of the subject had once been turned to studying the action of his mind, he began to recognize that the activity of his thoughts was not without an influence on the extent of the knee-jerk. It was soon noticed that noises which were not loud, and which could be only very weak sensory irritants, if of a kind to attract the attention, increased the extent of the phenomenon, while much louder sounds, if devoid of interest, had no appreciable effect. Thus, during the examination at 8 P. M., April 12, when the average knee-jerk was 29 mm., some one was heard coming up stairs, and the knee-jerks, which happened to be taken at the time,

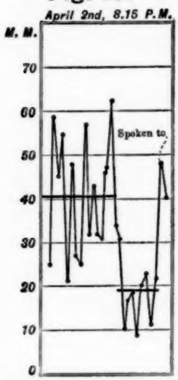
Fig. 9.
April 12th, 8.00 P. M.



were 71 mm. and 60 mm. (Fig. 9.) At the same time, the rattling by of carts, an accustomed sound, and one devoid of interest, had no appreciable effect. It was soon found that if the subject were spoken to, if a knock came at the door, or if in any other way the attention of the subject were attracted at the moment that the blow was struck, the knee-jerk was markedly increased.

Effect of Cerebral Inactivity and of Sleep. If the sudden awakening of the attention was capable of increasing the knee-jerk it might seem as if a quieting down of cerebral activity would produce the opposite effect, and this appeared to be the case. Not infrequently the average of the experiments at the beginning of an examination, when the mind of the subject, who had perhaps just stopped working, was in an active state, was considerably higher than the average of the experiments which were made toward the close of the examination, when quiet, or even a condition closely resembling sleep, had crept on. It is, perhaps, worth noting that the subject has always had the faculty of going to sleep at short notice, and that the jars caused by the regular blows of the hammer ceased to attract his attention after a few hundred experiments had been made upon him. The effect of the quieting down of the cerebral mechanisms was illustrated in the examination at 8.15 P. M., April 2, when the average of the first fifteen experiments was 41 mm. and the average of the next

Fig. 10.

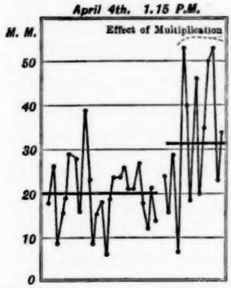


ten experiments was 19 mm. The next two blows were struck just after the subject had been spoken to, and the knee-jerks were 48 mm. and 40 mm. (See Fig. 10; see, also, Fig 9 and Fig. 6.)

Effect of Different Forms of Cerebral Activity.—The experiments which we made with reference to the effect of different forms of cerebral activity were far too few to offer a basis for positive conclusions, but it seemed to us that it was the emotional forms of activity which had the greatest influence on the process. Thus, in the case of mental arithmetic, the simple act of multiplying two numbers, even if they were difficult, did not seem to affect the knee-jerk especially, unless the endeavor was made to obtain the result as quickly as possible and the subject were excited by the attempt. The question is worthy of an especial research. One great difficulty in such a research arises from the fact that the experimentor cannot time the blow so as to get the knee-jerk at the moment when the mind of the subject is most actively employed. It is possible that such experiments might be combined with plethysmographic experiments to advantage.

Effect of Multiplication.—At the examination at 1.15 P. M., April 4th, we tried the effect of multiplication. The average knee-jerk at the time was 20 mm., while the average during the period

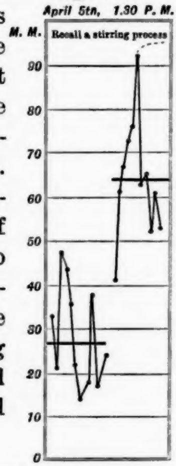
Fig. 11.



when the subject was rapidly multiplying, was 32 mm. (Fig. 11.)

REËNFORCEMENT CAUSED BY EXCITING MENTAL WORK.—A good example of the effect of exciting mental work is to be found in the results of the examination at 1.30 P. M., April 5th, when the subjet repeated to himself Browning's stirring poem—"How they Brought the Good News from Ghent to Aix." The average knee-jerk during the preceding quiet had been 27 mm., and thė average taken while the poem was being recalled to memory was 64 mm. (Fig. 12.) In such a case as this, one cannot help thinking that the muscles of the larynx may have been called into play, and that the rhythm of the respiration may have been altered. The subject was not conscious of making any attempt at phonation, but it did seem to him that his breathing had been longer and deeper.

Fig. 12.



EFFECT OF RESPIRATION ON THE KNEE-JERK.—It is interesting to consider, in this connection, the effect of the respiration on the knee-jerk. A few experiments were made with reference to this point, the respiration and the knee-jerk both being recorded on the same moving surface. It was not found in these experiments, however, that the res-

piration had any effect upon the phenomenon. It seemed to make no difference whether the blow fell at the beginning, middle or end of inspiration, or at the beginning, middle or end of expiration. In fact, as far as these experiments gave information, the regular acts of respiration do not reënforce the knee-jerk.

Renëforcement Produced by Asphixia.(?)—The following experiments show that the knee-jerk is not altered by slight changes in the respiratory rhythm, but that it is increased by violent respiratory movements, or the causes which produce them. In the examination made at 8.30 P. M., April 8th, the average knee-jerk was 51 mm. The following experiments were made fifteen minutes later, and in just the same way, except that the blows were delivered at intervals of ten, instead of fifteen seconds, the usual rate. The figures show, as in all other cases, the extent of the movements of the foot, resulting from the knee-jerk, in millimetres.

During quiet—35, 29, 55—a deep inspiration is taken, and the breath is held for seventy seconds—41, 44, 45, 49, 55, 72, 100—breathe again, and at first very hard—72, 57, 61, 42, 41, 52, 44, 32—Another deep inspiration taken, and held seventy seconds—56, 58, 67, 70, 78, 79, 89—breathe again, and heavily—80, 59, 64, 56, 41, 30.

The first time the breath was held, more than forty seconds elapsed before a material increase in the extent of the knee-jerk was seen, but during the next thirty seconds, when the endeavor to keep from breathing had become painful, the increase in the knee-jerk was very marked. As soon as the subject

began to breathe again the irritation began to pass off, and the movement to become less, and in about forty seconds it had got back to its normal average.

When the breath was held the second time, the increase in the knee-jerk came much sooner, and as in the first case, the extent of the movement increased as the feeling of oppression increased. As in the previous case, it required about forty seconds after breathing had begun again, for the knee-jerk to get back to its normal amount.

How far the increase in the phenomenon seen in these experiments was due to the pain, and how far to the effects of temporary asphixia upon the central nervous system, is difficult to say.

Similar results were got when the breath was, as far as possible, expelled and kept out. During quiet—52, 41, 47, 46, 41—breath expelled and kept out—65, 80, 85, 99—breathe again—72, 80, 60, 69, 63, 67, 44. This was a much more painful experiment, and the effect of the lack of air was perceptible almost at once in the increase of the knee-jerk. At the end of forty seconds the pain was so intense as to bring tears to the eyes, and even after the breath was taken again, the painful feeling referred to the lower part of the chest lasted for some time. It is noticeable that in this case the knee-jerk returned to the normal more slowly than in the previous experiments.

These experiments are recorded here not because any definite conclusion can be drawn from them alone, but because they are suggestive, and because they illustrate one more of the many sources of reënforcement of the knee-jerk. Whether they should be grouped with reënforcements which result

from painful sensory impressions, from voluntary actions, from emotional activity, or from functional disturbance of the spinal centers, is hard to say, since all these causes seemed to take part in producing the result.

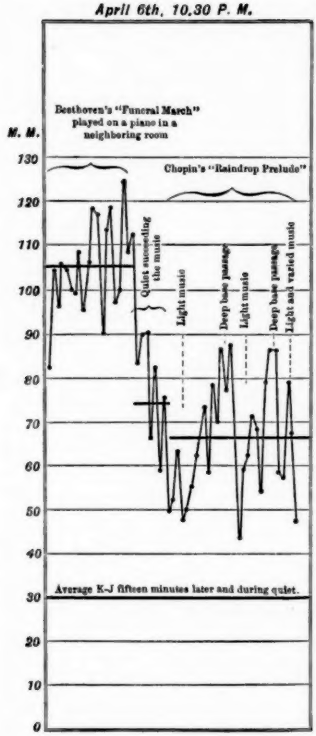
Reënforcement of the Knee-Jerk Caused by Music.—Perhaps the most interesting of all the forms of reënforcement attributable to cerebral action, which we saw, was that produced by music. Not all forms of music have this power, however, and, as far as we have been able to judge, it is confined to such as are capable of exciting an emotional interest. For instance, the writer can state that "Beautiful Spring," when played by a hand-organ, has little or no effect upon his knee-jerk, although a good military band, when playing a stirring march, is able to cause a very decided reënforcement.

One day during the experiments a procession passed the end of the street, a short distance away, and the effect of the music was very evident. The twenty-five experiments of the examination which had just been made had shown the average knee-jerk to be 32 mm. At the approach of the procession the subject resumed his place on the apparatus, but the first blow was not struck until the first band was passing the end of the street—60, 71, 74, 70, 60, 55—another band immediately followed, and it began to play "My Maryland" just before it reached the street—62, 76, 76, 74, 71, 66, 59, 64, 59—this was followed by a drum corps—48, 55, 51, 55, 53, 49, 52—and then the music died away in the distance and only the ordinary street sounds remained—40, 45, 37,

30, 39, 53, 37, 29. The increase and decline of the knee-jerk as the music approached and died away, and the difference in the effect of the bands, the drum corps and the street sounds, is very interesting. The fact that the character of the music determined its power to reënforce the knee-jerk was still more clearly illustrated in an experiment made on April 6. The average knee-jerk at 8 P. M. was 32 mm. and the average knee-jerk at 11 P. M. was 29 mm. It is fair to assume that at 10.30 P. M., the time of the experiment, the average knee-jerk during quiet would not have been far from 30 mm. The music used in this experiment was a good piano in a neighboring room, played by a skillful pianist. While Beethoven's "Funeral March" was being played the knee-jerks were, viz.: 82, 104, 96, 105, 104, 99, 108, 95, 106, 118, 117, 90, 113, 119, 97, 100, 124, 108, 112, and the average was 105. This was followed by an interval of quiet, during which the knee-jerks fell off—83, 90, 90, 66, 82, 59, 75, 50; average, 74. Then Chopin's "Raindrop Prelude" was played, and to our delight, when we came to read the results we found that the extent of the knee-jerk had varied with the character of the music in the most remarkable manner. Thus, during the soft music, when the raindrops are supposed to be falling, the knee-jerk was 52, 63, 47, 50, 55; as the music changed and the deeper passages began to make themselves felt, it was 66, 73, 58, 78, 70, 86, 77, 87; as the music subsided and became softer the measurements were 66, 43, 59, 62, 71, 68, 54; as the more thrilling passages succeeded 79, 86, 86 was measured, and finally, as the varied but softer parts came again, the knee-jerk was 58, 57, 79, 67, 47. As has been said, the average of the knee-jerk during

Fig. 13.

April 6th, 10.30 P. M.



quiet, as found by twenty-five experiments taken a short time after the subject had quieted down, was 29 mm. (See Fig. 13.)

Perhaps the reader is inclined to doubt that music could have had such an effect, and may wonder, as did the writer, whether it were not possible that the subject of the experiments had unconsciously favored, or, perhaps, even almost manufactured the results. That this was the case, however, scarcely seems probable, because the subject was never sure during the examinations of the extent which his foot moved, excepting to know that the movement was slight or was considerable, and he was unaware of the closeness with which the knee-jerks had followed the music until he saw the curves after the experiments were over. Had this been the first set of experiments which had been made on the subject it is probable that he would have been much more interested in the blows of the hammer than in the music, but as this was the sixth day of the series, and as his knee had been struck more than a thousand times during

the week, he was able to forget the blows of the hammer and to think only of the music.

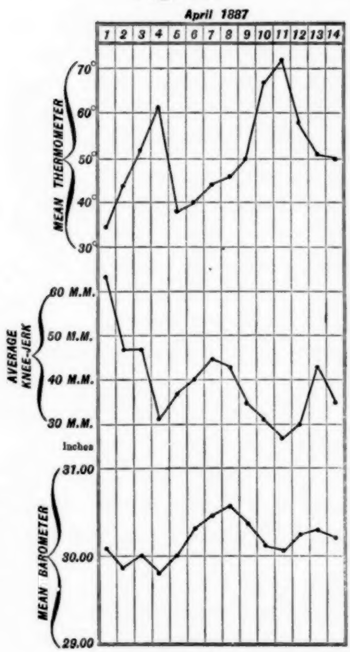
Reënforcements Produced by Exciting Dreams. The fact that the amount of the knee jerk is largely dependent on the activity of the cerebral centers, which are the seat of the emotions, has received another and curious illustration in the course of our experiments. As has been said, the subject, when tired, not infrequently dozed off toward the end of an examination, or, at least, so far lost consciousness that he became no longer responsible for his thoughts. Thus, it not infrequently happened that he pictured himself as kicking a football, or straining to lift a heavy weight, or steadying himself to aim a pistol, or as performing some other vigorous action, and if, as was not seldom the case, the blow on the ligamentum patellæ was struck at such a moment, he was recalled to himself by the unusual violence of the resulting knee-jerk. This was not a single experience, but happened many times, so that the subject had no doubt of the correctness of the observation.

Even during sleep, then, cerebral activity is making itself felt throughout the body. This fact scarcely needed a proof, for every one has noticed the running movements of sleeping dogs, etc. It is interesting in this connection, however, because evidence thus obtained is much more trustworthy than any which could be gained during waking hours, when the subject might be thought unintentionally to help to bring about the results.

Influence of the Weather Upon the Knee-Jerk.—In the course of the experiments the subject noticed that his general condition and his knee-jerk were be-

coming less vigorous and attributed the change to the fact that the weather was becoming warmer. The first warm spring days give most men a feeling of lassitude, and the subject knew that he was no exception to the rule. One's sensations are unreliable data, unless corroborated by more substantial evidence, and it seemed worth while to compare the recorded variations of the knee-jerk with the variations of the temperature during the two weeks. The U. S. A. weather observations were accordingly consulted, and it was found that, in general, as the temperature increased the knee-jerk became less. The correspondence was not so close, however, but that it was evident that other influences were at work, and it occurred to the writer that the barometric changes of the atmosphere might be of importance in this connection. How greatly the extent of the knee-jerk is influenced by thermometric and barometric changes can be best understood by study of Fig. 14.

Fig. 14.



on.—At the head of the chart is written the dates on which the experiments were made, and at the left side the

Farenheit thermometer scale from 30°–70,° a scale of millimetres, to show the extent of the knee-jerk, and the barometer scale from 29–31 inches. The curve opposite the thermometer scale shows the variations of the temperature, each of the dots connected by the lines giving the mean temperature for the corresponding day. The curve opposite the millimetre scale shows the variations of the knee-jerk, each dot representing the average of all the experiments taken on the corresponding day. Similarly the curve opposite the barometer scale records the variations of the barometer, each dot giving the mean of the barometer for the corresponding day.

STUDY OF THE DIAGRAM.—The correspondence between the temperature and the knee-jerk curves is not very accurate, but one sees that on the 4th and 11th, when the temperature was high, the knee-jerk was low, while on the 1st, 7th and 14th, when the thermometer was much lower, the knee-jerk was considerably higher. In general, then, as the temperature rises, the knee-jerk becomes less, and as the temperature falls, the knee-jerk becomes larger.

If now one compares the knee-jerk and barometer curves, he finds the agreement to be much closer. The barometer fell, roughly speaking, from the 1st to the 4th, so did the knee-jerk; the barometer rose from the 4th to the 8th, so did the knee-jerk; the barometer fell from the 8th to the 11th, so did the knee-jerk; the barometer rose from the 11th to the 13th, so did the knee-jerk; and finally, the barometer fell from the 13th to the 14th, and so did the knee-jerk. In general, then, it may be said that as the barometer rises and falls the knee-jerk rises and falls.

A more careful examination, however, shows that though this general correspondence existed, the two did not agree in the extent of their variations, nor did they vary in just the same way from day to day. Thus the knee-jerk fell markedly from the 1st to

the 4th, and the barometer fell only a little; moreover, the barometer rose from the 2d to the 3d, while the knee-jerk was stationary. Again, one sees that the knee-jerk fell off from the 7th to the 8th, although the barometer was still rising. These differences can only be understood by simultaneously comparing the three curves, and remembering that a rise of temperature or a fall of the barometer tends to depress the knee-jerk, while a fall of temperature or a rise of the barometer tends to elevate the knee-jerk curve.

From the 1st to the 2d the temperature rose and the barometer fell, and both of these influences acted to lessen the movement; from the 2d to the 3d the temperature continued to rise and the barometer rose, and the counteracting influences caused the knee-jerk to remain stationary; from the 3d to the 4th the temperature rose and the barometer fell, and the knee-jerk curve consequently fell very low; from the 4th to the 5th the temperature fell markedly and the barometer rose a little, and the knee-jerk began to recover; from the 5th to the 7th the barometer rose markedly, and the slight rise of temperature which occurred, not being sufficient to counteract its influence, the knee-jerk curve rose; from the 7th to the 8th the continually increasing temperature began to make itself felt, so that the process became less active, in spite of the fact that the barometer continued to rise; from the 8th to the 11th both the temperature rose and the barometer fell, so that the knee-jerk was greatly depressed; from the 11th to the 13th the temperature fell and the barometer rose, and both influences assisted to restore the knee-jerk; from the

13th to the 14th, however, the barometer began to fall again, and the temperature being nearly stationary, the knee-jerk was again depressed.

These curves show most clearly that the knee-jerk is closely dependent on changes in the weather, but, inasmuch as we are something more than weather-gauges, the variation is qualitative rather than quantitative. The fact that other influences are at work is shown in the course of the curve of the knee-jerk, when looked at as a whole. Thus one observes that the general condition of the subject, when looked at from this standpoint, was falling off during the two weeks, in spite of the fact that the barometer was, on the whole, rising: moreover, this depression of the knee-jerk would seem to be greater than the rise of temperature, which occurred during this time, could account for. The fact is easily explained; the work involved in the research and in the study of the records gained in the experiments was not small, and the fatigue which the subject felt at the end of the fortnight was an undoubted element in causing the marked falling off of the knee-jerk.

It is no new discovery that the general condition of man is greatly influenced by changes of the weather, but a demonstration of the fact is nevertheless valuable and may perhaps drive home the lesson already learned by physicians and surgeons in their practice.

It naturally suggests itself that what we call the weather is composed of other conditions beside those recorded by the thermometer and the barometer, and that the direction of the wind, the degree of humidity of the air and the electric potential of the atmosphere

may well have an influence upon man. There can be no doubt but that the degree of the humidity of the atmosphere influences us greatly by determining the evaporation of the perspiration from the skin, but that we are influenced by the electrical condition of the atmosphere is by no means as certain. One knows so little concerning the electrical changes of the air that the subject is always an attractive theme for speculation, and one is in the habit of holding it responsible, in a vague sort of way, for many peculiar feelings which he cannot otherwise explain. The idea is a popular one and even finds its way into the novel of the day. Thus, one reads: "I hastened to do as I was asked, the more readily as, what with fear and horror, and the *electric tension* of the night, I was myself restless and disposed for action." [1]

Plate II was constructed to enable one to readily compare the variations of the knee-jerk with the changes which all the different components of the weather underwent at the same time.

The study of the electrical condition of the atmosphere is a difficult one and requires the use of special appliances. The writer could, therefore, scarcely have gained any information on this point had it not been for the kindness of Mr. Park Morrill, who was making a special study of this subject near by, at the Johns Hopkins University. The curve of the electric potential of the atmosphere given in the chart is based on Mr. Morrill's figures, which state in volts the electric potential of the air as compared with that of the earth, regarded as 0.

Explanation of Plate II.—On the first line, at the top, are given the dates, and beneath them the hours of the day, from 7 A. M. to 11 P. M.

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[1] The Merry Men, by R. L. Stevenson.

The first curve of the chart shows the diurnal variations of the knee-jerk as determined by seven examinations. The average of all the experiments made at each examination is represented by a dot, placed at the proper height with reference to the millimetre scale, at the left side of the chart, and under the day and hour at which the examination was made. The lines connecting the dots enable the eye to readily follow the variations of the knee-jerk during each day. The larger dots represent examinations which were made directly after a meal.

On the base line of this knee-jerk chart is recorded the direction of the wind in the morning, afternoon and evening of each day.

Below are arranged, in order, the curves which show the variations of the barometer, the thermometer, the electric potential of the air, and the relative humidity of the atmosphere. Each dot in each of these curves represents a separate observation, and is placed at a height corresponding to the scale at the side of the latter, and under the hour at which the observation was made.

The heavy cross lines show the average knee-jerk for each day, and the mean of the barometer, of the thermometer, and of the electric potential of the air for April, as determined by the observations of a number of years.

STUDY OF PLATE II.—It seems to the writer that this chart is of great value from the negative evidence which it offers. It shows that a change in the direction of the wind, a change in the electric potential of the atmosphere, and slight changes in the relative humidity of the air, are without visible influence upon the knee-jerk, and, presumably, upon the central nervous system. It also calls the attention to the fact that the variations of the barometer and the thermometer, though of the greatest importance in determining the height of the daily average of the knee-jerk, are secondary to hunger and fatigue in their effect upon its hourly variations.

SUMMARY OF RESULTS OF EXPERIMENTS OF SERIES I.—The extent of the normal knee-jerk is continually undergoing change. So great are the variations, even when the subject is at rest, that a correct idea of the activity of the process can be gained only by averaging the results of twenty or more experiments. The average knee-jerk varies in amount at different

times of day, being as a rule greatest in the morning, soon after breakfast, and being very much less at night. The decline which occurs as the day advances is very irregular, but, in general, the knee-jerk is larger after each meal. Finally, the extent of the knee-jerk may differ greatly on different days.

The causes of these variations of the knee-jerk are not only alterations in the muscles and nerves involved in the process, but, to a still greater degree, changes in the activity of the central nervous system, either as a whole or in part. Thus fatigue, hunger, enervating weather and sleep, conditions which decrease the activity of the whole central nervous system, decrease the average knee-jerk, while rest, nourishment, invigorating weather, and wakefulness, influences which increase the activity of the central nervous system, increase the average knee-jerk. These influences account for the diurnal variations of the knee-jerk, while the multitude of changes that are seen to occur within short intervals of time are due to temporary alterations in the activity of certain parts of the brain and cord. Thus voluntary movements and strong emotions, when synchronous with the blow, are found to increase the movement; and this is noticed even during sleep when the dreams are vivid. Similarly, sensory irritations, even when not strong enough to produce visible reflex actions, may markedly reinforce the knee-jerk, but whether on account of their effect upon the brain, or upon the spinal cord, must be proved by future experiments.

Inasmuch as the normal respiratory movements and quiet thought were not seen to influence the process, it seems probable that the action of the many

mechanisms of the central nervous system, except when very strong, is not accompanied by the development of reënforcing influences; this is far from certain, however, and, inasmuch as the origin of but few of the more delicate reënforcing influences were discovered, this interesting question must be left open to future study.

In general, then, it may be said that the knee-jerk is increased and diminished by whatever increases and diminishes the activity of the central nervous system as a whole, and that it is even more noticeably altered by temporary changes in the activity of certain mechanisms of the spinal cord and brain.

In the experiments described in this paper it was found that the movements of the foot, caused by knee-jerks that were produced by the usual blow, *i. e.*, when the hammer fell through an arc of 40°, varied from 0 millimetres to 130 millimetres. Still greater movements would undoubtedly have been seen had vigorous rëenforcements occurred at the time when the average knee-jerk was higher. The average movement gained from the results of the 2,320 experiments of this series was forty millimetres. The least blow which was seen to produce a movement of the foot was obtained by letting the hammer fall through an arc of 20°.

The Results of Experiments of Series II.

The results of the experiments of Series I were so remarkable that it seemed to the writer that he ought not to publish them without assuring himself of their correctness. He accordingly undertook a second series of experiments, which extended like the first over two weeks, and which differed from

them only in this: that nine instead of seven examinations were made on each day. The two extra examinations were made, the one between eleven and twelve o'clock in the morning, the other, between four and five o'clock in the afternoon. These experiments were made with all the care that was given to the previous series, but it seems unnecessary to publish the results in detail. Suffice it to say that the conclusions reached in the second series of experiments corroborated those which were obtained in the first series in every particular. There were the same extraordinary variations in the extent of the knee-jerks produced at intervals of only a few seconds. The average knee-jerk was found to be highest soon after breakfast, and to be low at night, and it was seen to be higher after than before each meal. The extra examinations, made in the middle of the forenoon and afternoon, showed, moreover, that the average knee-jerk gradually fell throughout the forenoon and throughout the afternoon, unless some unusual counteracting influence prevailed. It was also found that the average knee-jerk changed from day to day, but the variations in the weather during this period were so slight that the other influences which determine the general condition of the individual were most active in determining the amount of the average knee-jerk. The average movement gained from the 3,156 experiments of this series was 33 millimetres. Finally, all the sources of reënforcement which were noticed during the first series were found to be active during the second.

As a proof of these statements the author appends a table which gives a summary of the results gained in Series II, the table being made on the same plan

as the table on page 39, which gives the summary of the results of Series I.

SUMMARY OF RESULTS OF EXAMINATIONS OF SERIES II.

May, 1887.	7-8	9-10	11-12	1-2	2-3	4-5	6-7	8-9	10-11	Average K.-J. in mm.	Total No. of Examinations.	Total No. of Experiments.	Mean Barometer.	Mean Thermometer.
9th..........	36	43	64*	52	54	39	49	31	41	45	9	225	30.012	65°
10th.........	47	60	50	41	41	55	28	39	29	43	9	227	30.190	65°
11th.........	39	53	24	31	26	28	26	24	28	31	9	239	30.009	70°
12th..... ...	23	44	31	31	36	20	23	30	25	29	9	218	30.002	71°
13th.........	37	51	27	14	25	38	23	29	14	28	9	246	30.005	66°
14th.........	39	54	30	24	16	37	25	27	35	32	9	228	30.310	60°
15th (Sun)...	38*	43	48	43	40*	...	18	29	37	37	8	211	30.230	66°
16th.........	26	46	54	24	35	37	25	35	32	35	9	229	30.080	66°
17th.........	29	46	43	31	46	19	30	38	24	34	9	225	29.910	68°
18th.........	36	37	24	33	36	33	45	39	37	35	9	227	29.850	69°
19th.........	33	52	33	35	25	25	26	29	37	33	9	228	30.020	72°
20th.........	38	33	36	25	25	33	33	25	21	30	9	228	30.020	72°
21st.........	36	38	37	26	23	8	14	26	7	24	9	224	30.170	72°
22d (Sun)...	26*	36	20	19	29*	...	9	22	16	22	8	201	30.160	70°
	34	45	37	31	33	31	27	30	27	33	124	3156	30.069	68°

*The examination was one hour late.

DERMAL SENSITIVENESS TO GRADUAL PRESSURE CHANGES.

BY G. STANLEY HALL AND YUZERO MOTORA.

Τῇδε δοκῶ ζητοῦσι φανεῖσθαι. ἀνάτη πότερον ἐν πολὺ διαφέρουσι γίγνεται μᾶλλον ἢ ὀλίγον;

'Εν τοῖς ὀλίγον.

'Αλλά γε δὴ κατὰ σμικρόν μεταβαίνων μᾶλλον λήσεις ἐλθὼν ἐπὶ ἐναντίον ἢ κατὰ μέγα.

Πῶς δ' οὒ;

PHAEDRUS.
Stallbaum, ed. IV, p. 160.

Fontana observed that when a very slight pressure was applied directly to an excised motor nerve it might be made to increase so gradually as to crush the nerve without causing its muscle to contract. Afanasieff and Rosenthal found also that temperature might be increased and decreased so gradually as to kill a motor nerve trunk without stimulating it. Ritter and others since have found that the electric current has no effect if the density of the current is made to vary slowly enough. Heinzman[1] undertook a more serious experimental solution of the question whether a thermal stimulus could increase so gradually as to be unobserved by the sensory nerves so that death would finally supervene without any movement of either resistance or escape on the part of the animal. Frogs were heated (*a*) locally with a leg in water gradually warmed, and (*b*) totally by sitting on a cork floating in a cylinder of water, though it was much harder to boil intact and normal than brained or reflex frogs without sensation enough to cause motion. Their sensory seemed to

[1]Weber die Wirkung sehr allmäliger Aenderungen thermischer Reitze auf die Empfindungesnerven. Archiv für die gesammte Physiologie. Bd. VI (1872) S. 222.

conform to motor nerves in this respect. Fratscher[1] repeated these experiments, heating very gradually, by means of a lamp applied to the small bulbous end of a tube communicating with the large vase of water in which the animals were exposed, and found he could even induce rigor mortis in normal frogs by immersing only a small portion of the body in the fluid. Acid and alkali stimuli he found might also be applied so gradually as to kill the tissues without stimulating movement. The researches of W. T. Sedgwick,[2] to whose discussion of the topic the reader is referred, seem to show conclusively that in the case of heat this cannot be due to a diminished irritability of the spinal cord by reason of the heat carried into it by the blood, and that organs with a basis of protoplasm cannot so far reverse its laws as to completely lose functional power with no preliminary phase of increased activity.

Quite apart, however, from the question of painless death in such cases the problem of the gradual differentiation of sensation, though so little explored, abounds in practical and theoretical implications of great interest, and a series of determinations was begun here in 1884 upon the pressure-sense according to the following method: A balance, devised and made expressly for this purpose, consisted of a solid iron base and a strong brass beam seventy-two centimetres long, hung on a steel edge and sensitive enough to be far beyond the limit of differential perception with the initial weights used. Along the whole length of the beam runs an edged iron plate,

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[1]Weber continuirliche und langsame Nervenreizung; Jenaische Zeitschrift. N. F. I. 1. (1875) S. 130.

[2]On the variation of reflex excitability in the frog induced by changes of temperature. Studies from the Biological Laboratory of the Johns Hopkins University, 1882. Page 385.

made very true, to serve as the track for a truck, from which was suspended a little platform to carry weights. To this was attached a long horizontal band running about the drum of a kymograph, which we used as a motor on account of its approximately uniform rate of motion, changes in the latter being found, by careful measurement, so small within the times we used that they could be disregarded. The contact of the knife edge, on which the balance was pivoted, with its support, the center of the pivots of the wheels on the truck, and the application of the force by means of the band, were all on the same level, and by this means the effects of traction on the free oscillation of the balance were so slight that sudden reversals of the direction of motion, which could be brought about instantly at any time by a key described in a previous communication [Mind, No. XL., page 557], did not sensibly affect it. The car, which, after careful experiments with flowing sand (which suggests how irregular the best hour-glasses must have been), was found to be much more reliable, may thus travel along the entire length of the beam, and bearing any weight placed on its platform, at any rate in which the drum can be set in motion, and a pointer which it carries may be made to pass over the divisions of the millimetre-scale on the track to the beat of a metronome. Certain suitable velocities and weights with the rate of increment of pressure per second were carefully predetermined. Under one end of the beam was a metalic button, any size of which could be used, which was covered with rubber to eliminate temperature sensations—a matter which, where the contact of such an arrangement is for so long times, must be con-

stantly regarded—by which the pressure was applied to the skin, and on the other end of the beam was a small table with fixed positions for counterweights, by which, together with the position of the car, which could be started at its full velocity at once, the amount of initial pressure was determined. To minimize oscillations the counterweight was removed by means of a cam.

The mode of making observations upon the volar tip of the index finger, *e. g.*, is as follows: The arm is rested on a comfortable support, the hand turned upward and the eyes closed. A special receptacle is made to fit the whole surface of the nail into which it is laid just under the button, which is brought down to within a millimetre of it by a screw supporting the other overweighted end of the beam. At a signal the counterweight is lifted by the cam, and after a fixed interval of from one to four seconds, during which all oscillations, if there be any, has ceased, by a turn of the key the car begins to move without noise or jar, and the differentiation begins, while the time, involving the amount of increase or decrease of weight, is recorded by a metronome till the percipient decides whether the weight is increasing or decreasing and signals to stop the apparatus, and says plus or minus accordingly. The wrong judgments by all observers throughout were found to be so very rare that they have been disregarded. The protocol thus gives us the point of application (commonly the tip of the left forefinger), the initial weight, the absolute amount of pressure increase or decrease per second, and the time required for a judgment. As the experimentations progressed the two chief causes of variation, viz.: changing degrees of attentives and of certainty, steadily diminished.

Table I.

	5	10	20	30	40	50	60	65	70	75	80	85	100	200	500
H. B. N........		16.05 +14.06 −18.10	13.08 +11.91 −14.23	14.26 +12.25 −16.12	10.16 +8.17 −12.	7.36 +7.23 −7.5				4.02 +3.54 −4.5	3.14 +3.17 −3.11	5.4 +5.3 −5.5	5.26 +4.28 −6.23	6.1 +5.4 −6.9	
H. N...........	17.4 +18.4 −16.4	13.37 +13.1 −13.65	12.3 +11.5 −13.1	10.8 + 9.9 −11.9	7.5 +6.5 −8.5	8.2 +7.9 +8.5				6.65 +5.7 −7.6			6.15 +5.1 −7.2	7.25 +6.1 −8.4	
E. H. B........	16.7 +15.8 −17.6	7.42 +7. −7.8	6.02 +5.7 −6.2	5.22 +4.7 −5.7	5.82 +4.9 −6.8	7.88 +6.8 −8.7				8.3 +8. −8.6			5.67 +5.54 −5.8	7.7 7.5 −7.9	
J. M............	15.95 +15.40 −16.30	9.05 +8.4 −9.7	11.05 +9.8 −12.3	7.5 +7.8 −7.2	10.05 +8.7 −11.4	11.4 +8.2 −14.6	10.45 +8. −12.9						12.45 +10.3 −14.6	10.75 +8. −13.5	
Y. M..........	22.1 +23.9 −20.3	16.1 +16.4 −15.8	12.12 +13.26 −11.04	8.23 +9.05 −7.4	10.41 +11.7 −9.12	8.11 +9.12 −7.1	7.3 +6.7 −7.8	6.96 +7.3 −6.7	6.88 +6.6 −7.4	7.28 +7.2 −7.3			8.77 +8.4 −9.15	8.4 +6.9 −9.9	10.21 +7.31 −13.12
C. H.....	6.85 +7.50 −6.2	8.05 +8.1 −8.	8.25 +7.5 −9.	8.75 +8.7 −8.8	9.5 +9. −10.	8.35 +7.5 −9.2				12.35 +10.10 −14.60			14.05 +13.3 −14.8	+13. +11.8 −14.2	

In the preceding table the upper horizontal line expresses the initial weight in grammes. The rate of differentiation per second is always $\frac{4}{125}$ of this. The numbers are seconds and fractions of seconds. Of the signs prefixed plus denotes increase of weight and minus decrease, the numbers above with no sign being the average of the two below. Each number is an average, of twenty single experiments. Thus, with an initial weight of five grammes, where the rate of differentiation would be 0.16 grammes per second, it takes Y. M. 22.10 seconds to make up his mind with confidence whether the change of pressure he knows from the signal is taking place, is an increase or a decrease, while J. M. decides in 15.45 seconds.

TABLE II.

	$\frac{16}{125}$	$\frac{8}{125}$	$\frac{4}{125}$	$\frac{2}{125}$	$\frac{1}{125}$	$\frac{1}{250}$	$\frac{1}{500}$
H.B.N.		3.28 +3.25 —3.31	7.36 +7.23 —7.50	9.36 +9.23 —9.50	15.29 +14.67 —16.00	20.36 +19.00 —21.83	
H. N.	3.70 +3.80 —3.60	5.55 +5.40 —5.70	8.20 +7.90 —8.50	8.15 +7.65 —8.65	12.50 +11.90 —13.10	21.50 +21.33 —21.71	
E.H.B.	3.05 +3.10 —3.00	5.40 +5.40 —5.30	7.88 +6.80 —8.70	8.48 +7.66 —9.23	9.96 +9.25 —10.60	14.68 +14.91 —14.46	
J. M.		6.45 +4.1 —8.6	11.4 +8.6 —14.6	9.9 +10.2 —9.6	11.7 +11.2 —12.2	21.2 +20.5 —21.9	80. +85. —75.
Y. M.	3.43 +3.44 —3.42	4.58 +4.93 —4.36	8.10 +9.12 —7.10	11.65 +9.88 —12.98	22.76 +21.00 —24.40	34.04 +31.60 —37.80	66.00 +67.00 —65.00
C. H.	4.40 +4.80 —4.00	4.85 +5.10 —4.60	5.25 +5.10 —5.40	5.85 +6.10 —5.60	7.40 +7.10 —7.70	7.60 +8.00 —7.20	

In Table II the initial or threshold weight is constantly 50 grammes, which from Table I seems about the most favorable for all individuals for further exploring the psycho-physic relation here, and the rate of differentiation varies from $\frac{16}{125}$ to $\frac{1}{500}$ of this threshold value per second, the numbers as before representing seconds and each expressing an average of twenty single records.

Table III.

	5	10	20	30	40	50	60	65	70	75	80	85	100	200	500
		5.14	8.37	13.69	13.	11.77				9.6	8.04	14.69	16.83	39.04	
H. B. N........		+4.5	+7.62	+11.76	+10.46	+11.57				+8.5	+8.12	+14.42	+13.69	+34.56	
		−5.8	−9.1	−15.47	−15.36	−12.				−10.8	−7.96	−14.96	−19.93	−44.16	
	2.78	4.28	7.87	10.37	9.6	13.12				15.96			19.68	46.4	
H. N...........	+2.96	+4.19	+7.36	+9.05	+8.32	+12.64				+13.68			+16.32	+39.04	
	−2.64	−4.37	−8.38	−11.42	−10.88	−13.6				−18.24			−23.04	−53.76	
	2.67	2.37	3.85	5.01	7.45	12.6				20.04			18.11	49.28	
E. H. B........	+2.63	+2.49	+3.65	+4.51	+6.27	+10.88				+19.2			+17.73	+48.	
	−2.83	−2.24	−3.97	−5.47	−8.70	−13.92				−20.64			−18.56	−50.56	
	2.55	2.89	7.07	7.02	12.86	18.24	20.06						39.84	72.	
J. M.......	+2.61	+2.69	+6.27	+7.49	+11.13	+13.12	+15.36						+32.96	+51.2	
	−2.46	−3.10	−7.87	−6.91	−14.6	−23.36	−24.77						−46.72	−86.4	
	3.5	5.15	7.75	7.9	13.32	12.97	14.02	14.48	15.41	17.49			27.87	54.4	163.36
Y. M...........	+3.8	+5.24	+8.48	+8.69	+14.98	+14.59	+12.86	+15.18	+14.78	17.28			+26.88	+42.88	+116.96
	−3.2	−5.05	−7.06	−7.1	−11.77	−11.37	−14.98	−13.94	−16.57	17.52			−29.28	−63.36	−209.92
	1.09	2.58	5.28	8.4	12.16	13.36				29.64			44.96	83.2	
C .H.........	+1.20	+2.59	+4.8	+8.35	+11.52	+12.				+24.24			+42.56	+75.52	
	− .99	−2.56	−5.76	−8.45	−12.8	−14.72				−35.04			−47.32	−90.88	

In Table III. the upper horizontal line represents initial weights in a series of observations, the differentiations being always $\frac{4}{125}$ of the threshold per second. The figures of the table represent the grammes and fractions of a gramme it was found necessary to add or subtract before the difference was perceived. This was calculated from the first table.

Table IV.

	5	10	20	30	40	50	60	65	70	75	80	85	100	200	500
H. B. N.		1.51 +1.45 –1.58	1.42 +1.38 –1.45	1.45 +1.39 –1.51	1.32 +1.26 –1.38	1.23 +1.23 –1.24				1.13 +1.11 –1.14	1.1 +1.1 –1.09	1.77 +1.77 –1.78	1.17 +1.14 –1.2	1.19 +1.17 –1.22	
H. N.	1.56 +1.59 –1.53	1.43 +1.42 –1.44	1.39 +1.36 –1.41	1.34 +1.31 –1.38	1.24 +1.2 –1.27	1.26 +1.25 –1.27				1.21 +1.18 –1.24			1.19 +1.16 –1.23	1.23 +1.19 –1.28	
E. H. B.	1.53 +1.52 –1.56	1.24 +1.25 –1.22	1.19 +1.18 –1.20	1.17 +1.15 –1.18	1.19 +1.15 –1.22	1.25 +1.22 –1.28				1.27 +1.25 –1.28			1.18 +1.18 –1.18	1.25 +1.24 –1.25	
J. M.	1.51 +1.52 –1.49	1.29 +1.27 –1.31	1.35 +1.31 –1.39	1.24 +1.25 –1.23	1.32 +1.28 –1.36	1.36 +1.26 –1.46	1.33 +1.25 –1.14						1.4 +1.33 –1.47	1.36 +1.25 –1.43	
Y. M.	1.7 +1.76 –1.64	1.51 +1.52 –1.50	1.37 +1.42 –1.35	1.26 +1.29 –1.23	1.33 +1.37 –1.29	1.25 +1.29 –1.22	1.23 +1.41 –1.25	1.22 +1.23 –1.21	1.22 +1.21 –1.23	1.23 +1.23 –1.23			1.28 +1.27 –1.29	1.27 +1.21 –1.31	1.31 +1.23 –1.42
C. H.	1.22 +1.25 –1.20	1.26 +1.26 –1.26	1.26 +1.24 –1.28	1.28 +1.28 –1.28	1.3 +1.29 –1.32	1.27 +1.24 –1.29				1.39 +1.32 –1.46			1.45 +1.43 –1.47	1.41 +1.37 –1.45	

The fourth table represents the ratio between the threshold and the numbers expressed in the third table. In that table it was necessary for an initial weight of 5 grammes to be differentiated to the amount of two and seventy-eight hundredths grammes in order that the difference should be perceived by H. N., and five is to this number as twenty to fifty-six hundredths, as is shown in Table IV.

TABLE V.

	$\frac{16}{125}$	$\frac{8}{125}$	$\frac{4}{125}$	$\frac{2}{125}$	$\frac{1}{125}$	$\frac{1}{250}$	$\frac{1}{500}$
H.B.N.		10.49 +10.4 —10.59	11.77 +11.57 —12.	7.49 +7.38 —7.6	6.12 +5.86 —6.40	4.07 +3.8 —4.36	
H. N.	23.64 +24.32 —23.04	17.76 +17.28 —18.24	13.12 +12.64 —13.6	6.52 +6.12 —6.92	5. +4.76 —5.24	4.3 +4.26 —4.34	
E.H.B.	19.52 19.85 19.20	17.28 +17.60 —16.96	12.6 +10.88 —13.92	6.78 +6.13 —6.38	3.98 +3.7 —4.24	2.94 +2.98 —2.89	
J. M.		20.45 +13.12 —29.46	18.24 +13.12 —23.36	7.92 +8.16 —7.68	4.68 +4.48 —4.88	4.24 +4.1 —4.38	8. +8.5 —7.5
Y. M.	21.95 +22.02 —21.88	14.59 +15.78 —13.95	12.96 +14.5 —11.37	9.32 +7.9 —10.38	9.14 +8.4 —9.76	6.8 +6.32 —7.56	6.6 +6.7 —6.5
C. H.	28.16 +30.72 —25.60	15.52 +16.32 —14.72	8.41 +8.16 —8.64	4.68 +4.88 —4.48	2.96 +2.84 —3.08	1.52 +1.6 —1.44	

TABLE VI.

	$\frac{16}{125}$	$\frac{8}{125}$	$\frac{4}{125}$	$\frac{2}{125}$	$\frac{1}{125}$	$\frac{1}{250}$	$\frac{1}{500}$
H.B.N.		1.21 +1.22 —1.23	1.23 +1.23 —1.24	1.15 +1.15 —1.15	1.12 +1.11 —1.13	1.08 +1.08 —1.09	
H. N.	1.47 +1.49 —1.46	1.35 +1.34 —1.36	1.26 +1.25 —1.27	1.13 +1.12 —1.14	1.1 +1.1 —1.1	1.09 +1.08 —1.09	
E.H.B.	1.39 +1.40 —1.38	1.34 +1.34 —1.34	1.25 +1.22 —1.28	1.13 +1.12 —1.14	1.08 +1.07 —1.08	1.06 +1.06 —1.06	
J. M.		1.4 +1.26 —1.43	1.36 +1.26 —1.46	1.16 +1.18 —1.15	1.09 +1.07 —1.10	1.08 +1.08 —1.09	1.16 +1.17 —1.15
Y. M.	1.44 +1.44 —1.44	1.29 +1.31 —1.28	1.27 +1.31 —1.24	1.18 +1.16 —1.20	1.18 +1.17 —1.19	1.13 +1.12 —1.15	1.13 +1.13 —1.13
C. H.	1.56 +1.61 —1.51	1.31 +1.33 —1.29	1.17 +1.16 —1.17	1.09 +1.1 —1.09	1.06 +1.06 —1.06	1.03 +1.03 —1.03	

The fifth table represents the same relations as the third, except that the calculation is based on the experiments of the second table, while the third table is based on the first. The sixth table represents the same relations as the fourth, except that it is based on the fifth as the fourth is based on the third.

These results are presented so clearly in tables IV and VI that graphic representation in terms of ordnates and abscissas is unnecessary. They are more nearly uniform with Y. M., H. N. and H. B. N., while the other three deviate more from these and from each other. A relation very inaccurately approaching the constancy expressed by Weber's law is obvious, but is not only inexact, but appears only within limits themselves also subject to wide individual variations. C. H. (of Tables I and IV *e. g.*) recognizes a

differentiation of a constant rate per second with from as little as five up to from fifty to seventy-five grammes as an initial weight, while H. B. N. does not reach any constancy with an initial weight less than that of the upper limit of C. H. The latter subject (C. H.) was however especially selected from a furniture factory as a polisher and sand-paperer of exquisite pressure sense. This wide range of individual variation, which may be caused by both culture and heredity, may be utilized by anthropological methods, but from the results of experiments in the field of the psycho-physic law most analgous to ours was perhaps hardly to have been expected, at least with students with fingers uncalloused by manual labor.

Compared with the sensibility to differences of pressure determined by the more faultess of the many experiments with the appreciation of weights successively applied, our results show on the whole less sensitiveness. In some cases a change of $\frac{1}{20}$ or $\frac{1}{30}$ or even less of the initial weight has been perceived while with us, under the most favorable conditions (which seem *e. g.* in Tables II and V to be when a variation of $\frac{1}{250}$ of the initial weight of 50 grammes occurred per second) the judgment responds to a variation of about $\frac{1}{12}$.

In the study of capillary blood pressure in the human skin made by v. Kries,[1] a plate of glass was applied to the dermal surface and its pressure regulated by weights suspended to it below, and the effect, measured by the amount of paling, observed. The different thickness, rigidity and vascularity of the skin, as well as the method of observation, made results by

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[1]Uber den Druck in den Blutcapillaren der menschlichen Haut. Ludwig's Arbeiten, 1875.

this method very inexact. Yet the great effect upon capillary pressure produced by raising or depressing the arm, though much less than would be caused by the different positions of the limb, according to hydrostatic laws, was so considerable as to suggest a precaution against possible errors which we observed by keeping the hand at the same relative altitude with reference to the rest of the body. Again, Fechner admits that the pressure sense is liable to errors in that the elasticity of the skin prevents the pressure upon the nerve in terminal organs from corresponding exactly with the weight laid on the skin. The depression of the skin touched by the button was measured by means of a cyclometer by Y. M. for various weights upon his own fingers as follows—

Weight.	Depression.
5 Grammes.	.2151 Millimetres.
10 "	.4992 "
20 "	1.0078 "
30 "	1.3310 "
40 "	1.6784 "
50 "	1.7187 "
100 "	2.7490 "
200 "	3.0616 "

With our apparatus the smallest initial weights used bring the button in contact with the skin over its entire surface, and pressure does not increase the surface of contact as would be the case with a larger button. Increasing weight depresses new skin from a wider and wider area around the surface of contact, and may change the distribution of pressure over this surface, especially as between its centre

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and edges. Time is also probably a factor of the amount of depression and expulsion of blood. With the largest weights and longest times used by us, however, there is no distinct indication of insensitiveness increasing with the gradualness of the increment that seems due to local anæmia by pressure. If sensitive human tissue can be crushed without pain by increasing the pressure gradually enough after the analogy of Heinzmann's and Fratscher's experiments with heat applied to normal frogs, or even unusually great pressure-differentiation can be made so gradual as to escape attention when especially directed to it, a different apparatus method of experimentation than that used in this series of observations is needed.

How, then, shall we explain the new relation that appears between the last two columns of Table II.? Here, when the rate of differentiation of a constant initial weight is $\frac{1}{500}$ per second the time is nearly double what it is for a rate of differentiation of $\frac{1}{250}$ per second for Y. M. and nearly quadrupled for J. M. If the law of constant increment held irrespective of time, the numbers in each column should be double those corresponding to them in the column before, which occurs in but one case and approximately only in a very few other sporadic cases. Indeed, even the results of the last column may possibly be sporadic. We should expect however a priori a point somewhere where an increase in the time of applying a differentiation would diminish sensitiveness for it, but that this is reached in the last two columns of Table II., the results are too few to make us certain. Another problem presented by Table II. is to account for the great obtuseness for

differentiations applied at a relatively rapid rate. J. M., *e. g.*, is nearly four times as sensitive to differentiation applied at the rate of $\frac{1}{250}$, of the original weight, as he is when it is applied at the rate of $\frac{8}{125}$ of it per second.

In the best psycho-physic experiments involving the comparison of two weights, they are applied successively, with a definite time for contact, interval of rest, etc.—the application of both weights occupying *e. g.* five seconds—and the attention is then directed to the task of comparing the impression superposed in memory. In pressure, as opposed to lifting tests, little attention has been paid to the speed of application and levitation of the weights. With the second weight we might conceive that cells excited by the first are reëxcited, a few being left out of function, or a few new ones excited, according as the heavier weight comes first or last. By this method of gradual differentiation, however, the acts of comparison and judgment must go on during the process of the change, and the more rapid it is the greater the distraction. The comparison is made between an initial pressure held in memory and a present changing sensation. If memory were merely a faint sensation rapidly loosing intensity, we should have a double differentiation. The remembered initial pressure would fade like an after-image, while the present pressure is constantly increasing, and the differential sensibility would be finer than in the old method. The fact that it is less so cannot be entirely explained by the time required to stop the apparatus after a judgment is made, for that reduces itself in our experiment nearly to the reaction time from ear to hand of the person con-

trolling the drum, for the subject under observation gave the signal to stop the apparatus as soon as he felt a judgment within his reach, as it were, and it was expressed and recorded later. It is obvious, however, that a part, perhaps considerable, of the apparent decrease of sensibility from rapid differentiation is due to this constant error, but not all. Besides the perception-time a longer time is required to relate the two impressions in consciousness. The mind, our subjects think, does not keep or have at any time an image or feeling of continuous increment or decrement, Continuity here seems an impossible perception. The attention rather singles out an instant or degree of pressure and compares it with another instant and degree of pressure still further past (and, in fact, not invariably the period of the initial weight), and an impression arises or does not arise, which it is perhaps quite as correct to speak of as a sensation of difference, with a tolerably clear threshold of its own, as a judgment. Indeed, it seems to be impossible to excite a sensation of continuous increment. Again, with certain initial weights and certain rather rapid rates of differentiation, it is hard not to believe that the sensation changes in quality as it changes in quantity, and it may be impossible, with different tactile organs or fibre-ends at different depths of the skin, to get a quantitive change of entire purity. It is hard, however, to resist the impression that, quite apart from these minimal and inconstant changes of quality, the attention finds it difficult if not impossible to grasp continuity in the form of quantitative or intensive change, but rather that the directness of a graduated series is the basis immediately given, and that continuity is derivative and inferred.

Constancy, or uniformity, (as distinct from continuity), of sensory increment is of course not to be expected here, for it is the stimulus that increases uniformly per second, and the sensation, according to the law of Weber, must increase more slowly. Each second of increase bears a constantly less ratio to the total pressure of the preceding second, and if the pressure is decreasing, is in a larger ratio to it. Thus, as the differentiation goes on, a longer and longer time is necessary to create a given ratio for increasing and a less and less time for decreasing pressures. This fact probably is the chief cause of the rather large average errors for increasing weight. The later seconds effect even less sensory modifications than the first. Both greater sensitiveness and less average error in time might therefore be expected from decreasing pressures. The figures show, however, on the contrary, less sensitiveness and no greater uniformity. The most obvious cause for this result is fatigue. The cells, relieved from the effects of pressure, have been excited longest, while the mind has less interest in vanishing than in augmenting impression, and it is harder to bring the attention to bear on them.

In an interesting study by F. C. Müller,[1] which was begun on the excised nerve-muscle preparations of frogs, but extended to motor and then to sensory human nerves percutaneously excited and pointing to a "neurophysic," in place of the psycho-physic law, the author conceives changed excitability as an essential property of sensation. In the experiments of Wedenskii, and especially of Bowditch[2], whose

[1]Physiologische Studien ueber Phychophysik. Archiv f. Anat & Physiol, 1886. Heft. IV.

[2]Science Aug. 27, 1886.

tests seem as conclusive as they are important, changes due to fatigue cannot be assumed for the nerve fibre, but must be limited to terminal organs, the blood supply of which, as we have seen, is reduced by pressure, and to central cells." Our experiments allow no interval for rest and increased sensitiveness between the two degrees of pressure, which give rise to the impression of difference such as intervenes in the application of two successive weights. Where the transition is directly from one degree of stimulus to another, with no temporal interruption, the process cannot be the same as when a period of rest intervenes, or even, as in Müller's experiments, where on the basis of the stimulus of a constant current another stimulus in the form of negative variation is applied. Another complexity, also tending to make decision hard and slow, is that there are really three degrees of pressure to be constantly borne in mind—the original pressure as well as the alternative of increase or decrease—while in the most approved application of Fechner's three methods the problem has but two terms. The method of middle gradation only admits of comparison even in this respect with ours. Thus, in fine, whether we look at the number of terms involved in each verdict of consciousness, fatigue, the nature of the mental activity involved, the results, or every detail of method, we have here a new standpoint for viewing psycho-physic relations, and few if any safe inferences from one to the other between the work of Weber and Fechner and their successors and ours can be trusted. We are here confronted with new problems of great range and importance, which the above preliminary results, very far from solving,

barely suggest. What is the ratio, *e. g.*, between increasing suddenness and decreasing weight in producing a given sensory effect? On the one hand the mind has a horror of what is sudden which may amount almost or quite to kataplexy, which knowledge of law and power of prediction serve to alleviate; and, on the other, great changes, if very gradual, are not only imperceptible, but can only be ascertained by indirect and often very circuitous inference. If we compare the conscious minds of men to balances, some tipping to a greater and some to a less weight, we can only reply to the question why they do not tip to still finer stimuli, like the millionth leaf in Leibnitz's forest, by saying that, on the one hand, a practical threshold relieves it from distractions and irrelevancies and favors concentration by abstraction, or else that nature, as it were, suspects consciousness, and that its too great acuteness has been a disadvantage, and that attention must not be too discriminative nor admitted to all spheres of life. It is at least impossible to see any more contradiction between the law of probabilities and what Fechner would call the threshold theory of life, than between the untonality of octaves played by the wind on an Æolian harp and the same octaves on a piano with a pure untempered scale. Consciousness, in some of its aspects, has an articulating habit of dropping the fingers down upon the strings instead of sliding them along.

The following table gives the result of a series of records with heavy initial pressure and slow rates of differentiation, these being the conditions most favorable to fine discrimination:

TABLE VIII.

	1000.			500.			250.			125.		
	+	0	—	+	0	—	+	0	—	+	0	—
Y. M.	100.	128.	128.	66.	76.	78.	20.	20.	21.	18.	21.	18.
M. G.	88.	92.	104.	38.	—	56.	23.	28.	26.	11.	13.	12.
J. T.	60.	96.	94.	26.	72.	44.	23.	29.	28.	10.	11.	11.
Average...	82.	105.	104.	43.	59.	60.	22.	25.	25.	13.	18.	13.

All the figures in this table are grammes. Those above are the four initial weights, and the rest are grammes of increase rest or decrease before the judgment was made, the rate of differentiation throughout being 0.4 per cent. of the initial weight per second, each figure expressing the average of fifteen single observations, and the grammes under each of the four middle zero columns expressing the differentiation that would have taken place if differentiation there had been. Here again there is a general approximation to a constant ratio. The differential sensibility is finer than with smaller initial weights. With all these weights, and especially the lightest, it takes much longer to perceive rest or a minus quantity. This is expressed less, however, in the table above than in the following table of errors—

TABLE IX.

	0 = +	— = 0
1000.......	4.	42.
500.......	40.	38.
250.......	24.	21.
125.......	9.	10.

The figures in the above table express the per cent. of mistaken judgments, (calculated for not far

from fifty single judgments each.) Rest, *e. g.*, is judged to be increase in forty-five per cent. of the cases with an initial weight of 1,000 grammes, and decrease is judged to be rest in forty-two per cent. of cases with its same initial weight. These may be called errors of overestimation, and all errors of underestimation are comparatively rare, as are errors of overestimation when decrease is judged to be increase. That we should be insensitive to decrease was expected from fatigue and expulsion of blood caused by so heavy weight. That rest should so often seem to be increase may be due to gathering energy of attention or perhaps to the progressive action of heavy pressures upon the circulation in the tissues beneath. The fact that we tend to judge even rest as increase seems here, at least, to have made the result indicate greater sensitiveness to increase than if it had been practicable to start the differentiation, to be judged on the basis of the presumptive, slight, constant decrease required to offset this tendency, and which would therefore seem to consciousness to be rest. This constant we designate as the *apparent pressure constant*, and its variation at a given second during the process of an observation we call the *pressure deviation* of that decrease.

In another series of observations the effect of negative pressures or pulls upon the skin of the ball of the left index finger were studied as follows: After a number of trials with various salves and plasters in the market, one was at length found that was sufficiently adhesive, and within the limits of fifty grammes would not crack or give in a way to afford an independent clue to sensation as most do.

The finger nail was then glued to its socket in a heavy block below, and the beam of the balance allowed to swing freely, till, controlled by the position of the car, it came to rest in such a place as exactly to touch, without pressing, the upturned ball of the finger to which it was then also firmly stuck. The car was moved so as to give a very slight but distinct pressure, and then made to travel slowly away from the finger by means of the drum till a sensation of negative pressure, or a pull upward, was detected. It then traveled back till a positive pressure could be felt. Each of these pressures was repeated ten times and then averaged, and then the average of these plus and minus averages taken. This latter might be expected to give the original position of equilibrium of the car empirically determined as above (provided, of course, that the skin is equally sensitive to a push or a pull). The apparent was found, however, to be slightly more negative than the empirical indifference point, determined as above, in each of four subjects. As the pressure gradually changes from a minus to a plus quantity, or conversely, the neutral position is tolerably well marked to consciousness. The sense of contact is present, but without appreciable pressure or pull, as the finger is not absolutely flat even over the small surface of four or five milimetres in diameter, and as tactile experience is rarely with surfaces curved exactly conformably with the shape of the epidermis at rest, as it so nearly is in this case. A sensation of touch over such a surface, which in common tactile experience is impossible without pressure, might be expected to suggest pressure here. Possibly it will be found that it is in-

stinctive compensation for this association that makes a discrepancy between the *real and apparent tactile zero,* as we shall hereafter designate the mechanical and the sensory indifference points, respectively. Even should our further studies find them to coincide, it will be useful to retain both designations.

The apparent tactile zero thus determined is the starting point of our differentiations. The car is drawn to the position corresponding to this position of least sensation, and the percipient, after five seconds rest, hears the signal announcing the start of the car, and is to judge as soon as he can whether the skin is pressed or pulled upward. The sensations for a time are surprisingly indistinguishable. For a moment the change seems decidedly plus, an instant later it appears as certainly minus. The experience is comparable to that of binacular rivalry, where now the picture or color presented to one eye, now that before the other seems to predominate and indeed suggests quantitative determinations in this latter field. For this phenomenon we suggest the name of *antinomous dermal rivalry.* How far, if at all, this may be connected with the fact that every pull depresses the adjacent skin on the sides of the finger (which parts of the skin pressure distends), further studies must make known. On an average finger, disposed as above, a pull of *e. g.,* 20 grammes elevates the skin about three-fourths as much as the same weight depressed it. By referring to the preceding table of cyclometer measurements for the latter, it will thus be seen that in these determinations the beam of our balance has a movement of several millimetres, and emperical deter-

minations showed us that a slight but constant allowance must be made in the weights of the following table for overcoming the variation of the beam of the balance to even these slight changes of position. Making these deductions, we have the following sample of a single day's observations:

TABLE X.

	6.9.		2.6.		0.2.	
	+	−	+	−	+	−
G. S. H.....	12.7	13.3	....	....	7.9	6.9
Y. M.......	16.1	14.4	14 2	12.1	7.3	5.
E. C. S.....	13.	14.3	11.	16.9	8.8	1.2
Average....	13.9	14.	12.6	14.5	8.	7.9

In this table the figures at the top express the amount of differentiation per second in grammes. The figures below are grammes before the judgments of positive or negative pressure, expressed by the signs plus and minus above, were made. Each figure in the table is an average of ten single observations. The numbers are slightly too large, for they represent almost continuous observations with an element of fatigue distributed about evenly, but not eliminated as in fuller tables reserved for the completion of our research on this part of the subject.

The first and chief result of this table is the relatively vast weights involved in differentiation. Aubert and Kammler found the smallest weights that could be perceived when applied to volar finger tip to be from 0.005. to 0.015. gms., and Goldscheider's touch-points are probably at least no less sensitive. Though he did not control the amount of pressure

which he used in determining his pressure points, he was led to distinguish between the sensation of contact and that of pressure,[1] and found even the latter exceedingly sensitive. The literature relating to the psycho-physic law contains almost no reliable tests of pressure between the first observable contact and weights of from six to ten grammes. Strictly speaking, moreover, the sensation of an upward pull upon the skin must not be compared with pressure from within outward along the arterial tracts, or as shown in plethismographic tracings, nor inflammations or throbing sometimes called pounding pains. Nor is the collapse of supportive tissue beneath, which has been suggested as a cause of abnormal dermal sensations, more relevant than the sensations of the elastic skin artists who pull out folds of their skin into dewlaps. In fact, whether negative pressure, although it must favor a different distribution of capillary circulation from a pressure on the same spot, excites any specific sensation other than that of contact (which it may serve to show is specifically different from pressure), and secondary depression by stretching of adjacent dermal tissue, it is idle to conjecture.

On the whole, then, it may be said that, save the older determination of the smallest observable pressure from different parts of the dermal surface, and which since the works of Goldscheider need to be carefully revised, we know at present almost nothing with certainty about pressures below five or ten grammes. As we approach minimal pressures we pass outside the limits of validity for the psycho-

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[1]Neue Thatsachen über die Hautsinnnerven; Arbeit für Physiologie, 1885. Supplement-Band, p. 88.

physic law which has prompted most of the modern work in this field. Yet precisely within this realm covered by antinomious dermal rivalry lie the mysterious conditions of the tickle-sense by contact as distinct from the specified and localized tickle-sense, of abnormal excitability passing spontaneously into excitation where we have not yet learned to distinguish subjective from objective sensations, and with respect to which the mind of the adult is still in a rudimentary infantile condition. We also observe here that within these limits the slower differentiations are more finely distinguished. A definite law with regard to the comparative sensibility to pressures and pulls is not yet apparent.

When one index finger is under the button of each end of the balance so that the weight decreases on one finger as it increases on the other, there is no essential increase of sensitiveness, and in some cases a decrease. It requires some time and effort to accommodate the attention alternatively from the finger of one hand to the corresponding finger of the other.

A METHOD FOR THE EXPERIMENTAL DETERMINATION OF THE HOROPTER.

BY CHRISTINE LADD-FRANKLIN.

If the diagram of Plate III. be held in a horizontal plane in front of the face, with the arrow directed towards the bridge of the nose, and at such a distance that the circle, if produced, would go, roughly speaking, through two points a little below the centres of the eyes, an optical illusion will present itself. If one looks at the intersection of the middle cross, there will still be seen a cross on the plane of the paper, but there will be seen in addition a third line, which, if the paper is at the right distance from the eyes, will seem to stand up in a nearly vertical position, half above the plane of the paper and half below it. When the position is right for the middle cross, it is also right for all the others, and if the eyes are converged steadily upon the middle stick, the other crosses will also present nearly vertical sticks, visible by the lateral portions of the retina. The phenomenon is also pretty well preserved if the point of fixation wanders from one to another of the circular row of sticks. If the paper is gradually moved farther away from the eyes, the illusory stick may be made to look exactly vertical, but the position is not then quite right for the lateral portions of the field.

There are two points to be explained in this illusion: the presence of the third line and its upright position. Take a single pair of crossed lines, as in Fig. 2, hold them in a horizontal plane, and at such a distance that with the right eye shut, 1, and with the left eye shut, 2, looks like the projection of a vertical line. Now, with both eyes open, fixate a point at some distance beyond them (by sticking in a pin at that point if necessary). The lines will be seen double, as two entirely separate crosses. Let the point of fixation approach nearer to the intersection of the cross, and the double images will be brought nearer together until they partly overlap, and the appearance of Fig. 3 will be produced, where the image seen by the left eye is drawn in dotted lines, and the image seen by the right eye in uninterrupted lines. As the fixation point is brought still nearer to the intersection of the cross, the left-eye image of line 1 and the right-eye image of line 2 (which are parallel if the card is held at the right distance) come still nearer together, until the intersection is fixed and they exactly coincide. At the moment that they coincide, they leave the plane of the paper and become a single line in space, its lower end directed more or less exactly towards the feet of the observer. (Its exact position depends upon the position of the apparent vertical meridians for the given fixation point, which is different for different individuals.) As R_2 and L_1 unite, R_1 and L_2 present the appearance of a cross with the vertical line pass-

Fig 2.

Fig 3.

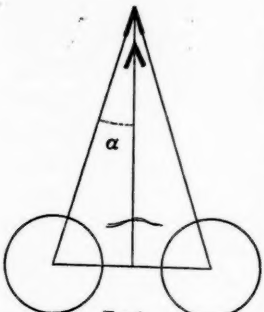

Fig. 4.

ing through their intersection. If we apply the construction for the cyclopean eye,[1] what takes place will be represented by figures 4 and 5. Fig. 4 gives the position of the eyes and of two pairs of lines, cutting respectively in the fixation point and nearer than the fixation point. (For simplicity only the near half of each cross is drawn.) The picture seen by the right eye, shifted through the angle α gives the right hand half of Fig. 5, and in the same way the right eye's image furnishes the left hand half. The cyclopean eye, then, sees two parallel lines coincident when, and only when, the fixation point is at the intersection of a cross. It will be noticed that the angle seen between the lines of the cross is twice as great as the angle drawn.

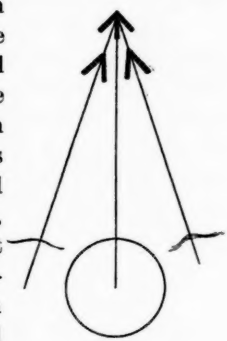

Fig. 5.

This illusory line is seen as one, then, because images of two different lines fall upon corresponding rows of points in the two retinas. The reason that it seems to be nearly vertical is that the only line in the median plane which is capable of throwing its images upon corresponding rows of points is the

[1] Hering: Beiträge zur Physiologie, I., p. 43. 1861.

nearly vertical line. Look at a single line drawn in the median plane upon a sheet of paper, held near the eyes and horizontally before them. To the right eye alone its near end will seem shifted towards the left, to the left eye alone towards the right; it is only when the plane of the paper is directed towards a transversal line through the feet that the given line seems to either eye alone to be in the median plane. It is imposible that any single real line should throw its images upon the apparent vertical meridians unless it is in the intersection of the planes through those meridians respectively and the fixation-point. If images are artificially thrown upon those meridians by two different but exactly similar lines, the mind, which is entirely unaccustomed to having such tricks played upon it, cannot escape the conclusion that it is looking at a single line in that position. The illusion is a remarkably persistent one; no degree of clearness of understanding of its origin will enable one to avoid thinking that the middle stick is out of the plane of the paper, provided he has good double vision, and has the power of steadily fixing the intersection of the cross. But this two-eyed illusion is very little in need of explanation after it has been noticed that there is a corresponding one-eyed illusion. In Fig. 6, the lines are all drawn so as to pass through a common point.

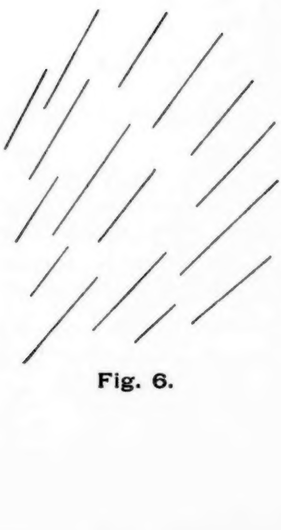

Fig. 6.

With a little trouble, one eye can be put in the position of this point,—it is only necessary that the paper be held so that, with one eye shut, the other eye sees all the lines leaning neither to the right nor to the left. After a moment, one can fancy the lines to be vertical staffs standing out of the plane of the paper. Fig. 7 is a modification of Zöllner's pattern, and, if looked at in the ordinary way, presents his well-known illusion. The short lines are not parallel, however, but each set passes through a point outside of the paper. If the paper be held horizontal, and if one eye be shut and the other put in the place of one of these points, the lines going through it will seem to be vertical, the other lines remaining horizontal. If the eye be now put in the place of the other point, the vertical and the horizontal lines will change places. I put a sheet of paper like this in position one day before the eye of a little girl eight years old, and asked her what she saw; she said a once, "I see two fences and two railway tracks."

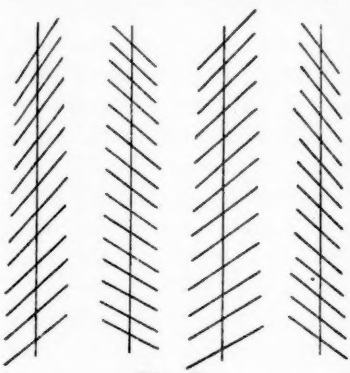

Fig. 7.

This illusion I take to have a purely mental origin. When a line lies anywhere in a plane through the apparent vertical meridian of one eye, and is looked at with that eye only, then, if we do not know how long it is and if it does not present any characteristic reflections, we have no very good means of knowing

how it is directed in that plane,—the only means we have, in fact, is the amount of change of accommodation that takes place as we look from one end of it to the other. Now of the lines in nature which lie anywhere within such a plane, by far the greater number (trees, edges of buildings, flag-staffs, pendulums, &c.), are vertical lines ; hence we are peculiarly inclined to think that a line which we perceive to be in such a plane is a vertical line. But to see a lot of lines at once, all ready to throw their images upon the apparent vertical meridian, is a thing that has hardly ever happened to us, except when they have all been vertical lines. Hence when that happens, we have a still stronger tendency to think that what we see before us *is* a group of vertical lines.

This illusion in regard to vertical lines is sometimes met with in nature. If one looks through a narrow tube at a small portion of china-matting, the straws of which run towards the feet, it cannot plainly be made out to be horizontal. There is a picture by Boughton in Mr. Walters' gallery in Baltimore, in which the paint which represents the surface of water is laid on with vertical strokes of the brush. If it be looked at with one eye, and with the hand held so as to cutoff the adjoining shore, it looks much more like a vertical wall than a level surface of water.

If, when looking at the one-eye lines, both eyes are suddenly opened, the sticks are instantly thrown down. In Fig. 7, however, the double images of the lines can be separated after a few minutes, and the appearance of vertical lines crossed by others is presented. But although the head be kept perfectly motionless, the vertical lines are tipped a little out of

their former position. The same effect is still more noticeable if two long parallel lines, at the exact distance apart of the eyes of the observer, are held horizontally before the eyes. To either eye alone, if the other be shut off by a screen, one or the other line looks perfectly vertical; but, with both eyes open, as soon as the vertical lines are distinguished, they are seen to have their nearer ends brought nearer together. This shifting may also be produced by forcible convergence of a shut eye, and an easy modification is thus furnished of a more difficult experiment of Le Conte's (*Sight*, page 186). It shows that though one eye looks at a near point, the outward rolling of convergence does not take place if the other eye is at rest.

In the two-eyed illusion of Plate III. all the lines are drawn so as to pass through one or the other of two points on the circle produced, and at a distance apart equal to the average distance between the two eyes. When the eyes take the place of these points, each eye sends to the brain information of a vertical line at the intersection of a cross, and their combined testimony is too strong to be in the least shaken by the knowledge that no such line exists.

If one has experience in uniting double images, the diagram may be held in various different positions, and a single line, variously situated in space, may still be recomposed. If it is held nearer to the eyes, the line declines into the plane, and if farther away, it becomes exactly vertical. If it is rotated in a horizontal plane, the line sinks down into coincidence with one branch of the cross, to rise again and fall into the other branch. If it is rotated into a vertical plane, the line points forward on top.

Looked at from underneath, the line is inverted; its top has now a slight indistinctness, which its bottom had before, for its top comes from the near portion of the cross, and accommodation becomes defective more rapidly coming in than going out.

When the line looks vertical it is not seen single throughout, although, if it is short, one is not easily aware of that fact. Support the plate on a table nearly on a level with the eyes, and fix the teeth in a head-holder[1] at such a distance as to make a line look vertical; its top may now be pricked in two by the point of a cambric needle; this cannot be done if the line is directed towards the feet. The divergence of its images is, in fact, the mark by which we know its degree of verticality. An actual vertical stick we see double at top and bottom, if we look at the middle of it, but our fingers have convinced us in so many millions of instances that the stick is not split, that we have come to quite overlook the visual splitting as splitting, but to give it its full significance as *sign of a vertical line*. It is really perceived, though not for itself, but only as part of a sensation-complex.[2]

This illusion derives its chief interest from the fact that it furnishes a very delicate means for determining whether we see double or not. When, in Fig. 2, the fixation point is near the actual intersection of the cross, the pair of parallel lines appear one on either side of the intersection of the imaginary cross, that is of the lines L_1 and R_2. When the fixation point is very near, the parallel lines are too

[1]Helmholtz, Physiol. Optik, p. 517.

[2]Stumpf: Ueber den psychol. Ursprung der Raumvorstellung, p. 270.

close together to be distinguished as separate lines, *but it can still be detected that the one which is seen is not at the intersection of the apparent cross*, and that is sufficient to show that the actual intersection is seen double. In the drawing of Plate I., the circle represents the theoretical horopter circle, which passes through the fixation point and the points in the eyes in which the sight lines intersect. The sight lines (*visirlinien*) are lines through points which appear to be in the same straight line—that is to say, the centres of whose diffusion circles coincide. They all cross in a point, which is in the image of the pupil formed by the cornea, and about 4 mm. in front of the mean nodal point.[1] The drawing must be supported on a horizontal table, and the head must be in a comfortable position and such that to one eye one set, and to the other eye the other set of lines lean neither to the right nor to the left. (If that cannot be done, it is because the drawing does not fit everybody's eyes). Some of the crosses do not cut on the circle. If one fixates one that does, and attends to the image in the lateral field of one that does not, then the latter can be made out to present the appearance above described. One sees now one and now the other of the vertical parallel lines, riding now on one and now on the other of the legs of the cross, and although one does not *see* the intersection double, one *infers* that he sees it double from the fact that he sees an apparent intersection with the line not going through it. So slight a separation of double images as this, one is quite unable to detect by any of the ordinary means. The effect

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[1]Hering, Phys. Optik, p. 466.

can be obtained, for a certain distance around, by a person who has good control of his attention, but has had no experience at all in optical experiments; he can, at least, perceive that if he looks hard at the stick on the right of the middle one, for instance, the perfection of the illusion for the stick on the left is quite broken up. But the tangent to the circle at the first goes through the second; hence he has proved that the locus of points seen single is not the straight line tangent to the horopter circle at the fixation point. The imaginary sticks form sufficiently interesting objects of attention to enable one to fixate them without any trouble. They also serve to take the place of a head-holder. The drawing can be made at ease with a circle of any convenient radius, and with the distance between the fixed points calculated for the observer from his interocular distance and the given convergence. The head can then be got into the required position and held there simply by the appearance of the lines.

This diagram, then, is sufficient to prove, even to the inexperienced observer, that the horopter is a circle when the fixation point is median and nearly in the primary plane. The experiment may be varied by having movable crosses which can be shifted about on the plane of the paper, but in that case the angle should also be made capable of being changed, which can be done if it is made by threads wound about a bit of cardboard. To determine the points seen single very far around in the lateral field, something brighter would have to be substituted for black lines—flat strips of platinum made white hot by an electric current, for instance. I have not yet carried out this experiment. The lines suffice for a

point-to-point determination, of from twenty to thirty degrees at a time, and this would amount to exactly the same thing, if the horopter circle went through the centres of rotation of the eyes. As it is, a motion of the fixation point along the circle, as drawn, changes a little the position and the size of the horopter circle. There are, of course, difficulties in the way of carrying out the test for remote portions of the retina. Besides the difficulty of seeing anything distinctly, there come in differences of perspective, and hence of the apparent size of an object large enough to be seen at all; the error of accommodation, which is particularly great for vertical lines;[1] the inclination of the horizontal meridians for near convergence; and the difference in strength between the nasal and temporal halves of the retina, which Schön has shown to be a factor of critical importance in all phenomena of double vision.[2]

The best experimental determination of the horopter which has hitherto been made is that of Schön (l. c). He arranges two openings in screens with lights behind them in such a way that lines of direction cut on a point of the horopter circle, and the image of an opening is then perceived at that point. I have repeated this experiment, but I do not find that the single image cannot be got when the point of convergence changes within certain limits. The same result can be more simply accomplished by a row of strings, with weights on the bottom of them, suspended from a rod. The points of suspension for

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[1]Fick, Physiol, Optik, p. 80.

[2]Archiv f. Opthalmologie, XXII., 4, p. 31, and XXIV., 1, p. 27 and 4, p. 47.

any observer may be determined beforehand, by drawing, in the way made clear by Fig. 8. The

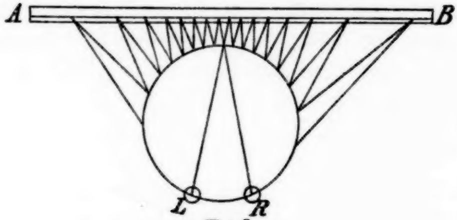

Fig 8.

strip of paper AB is cut off and nailed on to a strip of wood, and the points on it determine the points of suspension for the pendulums. To avoid the error produced by the opposite obliquity of vision of the two eyes, the half strips of paper and of wood should be inclined at an angle equal to the supplement of the angle of convergence (Fig. 9). When the eyes are brought into the right position, the strings can all (with the exception of the two outer ones) be brought into a cylinder of startling reality; after a few moments, their minutest fibres can be seen as distinctly as if one were looking at a cylinder of actual strings. *But it is not necessary that they should be at their constructed distances apart.* If they are hung at equal intervals, for instance, they are just as easily brought in, but they appear then in the shape of a plane. In this way they constitute a form of a well-known and much-discussed experiment.[1] All but the one looked at of the phantom

Fig 9.

[1] Hering, *Physiol. Optik*, pp. 398-403. Helmholtz, *Optique Physiologique*, pp. 827-835. LeConte Stevens, *Am. Jour. of Science*, 3, XXIII, p. 298. The slight curvature of the plane appears in the actual strings as well as in the phantom ones.

strings are now seen double, *but it is no easier to distinguish that they are seen double than it would be if they were actual strings.* They may be hung at various slightly irregular positions, and they then form various irregular surfaces, but there is no reason for saying that one rather than another of these various surfaces is a *Kernfläche* (Hering, l. c). It is even possible to set one of them to vibrating from side to side between its neighbors, without being able to perceive that one is seeing double. What happens then, is represented in Fig. 10. As the actual string A moves from *a'* to *a''*, the two phantom strings which it assists in forming make long excursions in and out, as is indicated for one of them by the two small circles of the figure. It is plain then, that the difficulty of making ourselves conscious of what points in space we see absolutely single is not at all obviated by this device. The same thing might be concluded from the fact that it led Schön to see the horopter of points as a circle, and Hering to see the horopter of upright lines as very nearly a plane. That difficulty is quite overcome, however, by such an arrangement as that illustrated in Plate I. I propose to apply it to the determination of the horopteric curve for non-primary positions of the fixation-point.

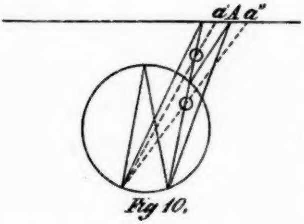

Fig 10.

THE PSYCHO-PHYSIC LAW AND STAR MAGNITUDES.

BY JOSEPH JASTROW, PH. D.

The application of the psycho-physic law to the relation between the estimated and the photometrically measured brightness of the stars has good claims to rank at once as the most practical, most important, most interesting and historically valuable illustration of the significant natural fact which that law formulates. The magnitudes were assigned to the stars at a time when no objective method of measuring the light emitted by them existed, and the stars were thus graded because that seemed the best way of arriving at a roughly quantitative notion of their relative illuminating powers. The eye was used as a natural (psychical) photometer, and now that artificial (physical) photometers rearrange these magnitudes, it is possible and important to trace the (psychophysical) relation between these two photometric scales. In fact, this very relation (with the exception, perhaps, of the physiological researches of E. H. Weber) did most to suggest to Fechner the formulation of his law. As he pointedly remarks, in this field the psycho-physic problem was solved before it was stated.

It will perhaps be well, before passing to the magnitudes themselves, to illustrate by an analagous instance what the psycho-physic relation in question

really means. For this purpose the historically first suggestion of the psycho-physic law, dating from Daniel Bernoulli (1730 or 1731) and elaborated by Laplace, will be the best. Bernoulli introduced into the calculation of probabilities the distinction between the value and the emolument of money. By the first he meant the buying power of the coin, by the latter the amount of the additional pleasure, comfort, etc., the money could bring in any one case. In other words, while a dollar will buy for A as it will for B, C and D the same amount of sugar, or of bread, yet the real value of that dollar will be much more to B than it will to A if B is a poor man and A a wealthy one. If A were to find a dollar on the street it would produce in him only the very slightest, if any, addition of pleasure or satisfaction, while if B found the dollar it would mean to him a very great happiness-increment indeed. To get a proportionately equal pleasure A would perhaps have to gain ten thousand dollars by a rise in his railroad stocks. The notion that underlies these commonplaces is that the amount of pleasure, the import of an addition of wealth, depends upon the wealth already possessed, being greater when that is less, and less when that is greater; and the most plausible supposition is that, *cæteris paribus,* (for one's liberality or avarice, and a hundred other circumstances, can alter this) the import, that is, the emolument, is inversely as the wealth. If my fortune amounts to $5,000 and my neighbor's to $10,000, an additional $500 is worth *twice* as much to me as it is to him; to have an equal increase he must get $1,000 when I get $500, in which case our fortunes are increased by one-tenth their whole

amount. Hence "equal emoluments" means "equal *ratios*" of the original wealth. Finally, suppose A has $1,000 and I give him $100; I now want to again so increase his fortune that he feels himself as much benefited as he did by the first increase; that is, I want to give him an equal ratio of his fortune, or an emolument equal to the first. To do this I must give him $110, and to give him a third "equal emolument" I must give him $121; and for a fourth, $133.10; for a fifth, $146.41, and so on. To produce an arithmetical series of 1, 2, 3, 4, etc., equal emoluments, I need a geometrical series of money-quantities, and the function expressing the relation of an arithmetical and a geometrical progression, that converts multiplying into adding is the *logarithm*. Hence we may say that the emolument is the logarithm of the wealth; and by widening the conception of the wealth to the general one of a physical stimulus of any kind, and similarly putting sensation in general for the particular sensation caused by an increase in money, you have the psycho-physic law. The practical difficulty is to *prove* that an increase of stimulus has always the same effect when it forms an equal part of the stimulus already present, instead of assuming it as was done above.

In the stars we have a large number of stimuli of all variations of intensity, and to introduce order into this series we roughly divide them into classes or magnitudes. This classification dates from Hipparchus, (about 150 B. C.), who happened to choose six such magnitudes, to one or other of which every star visible to the naked eye could be assigned. The stars of the first magnitude, by their preëminent

brightness, probably first attracted the attention, and got to be first enumerated; then in a descending scale the second, third, fourth and fifth, leaving all the faintly visible stars for the sixth. The magnitudes were determined presumably with the intention of making as much apparent difference *in toto* between one magnitude and the next above it, as between it and the next below it. That Hipparchus's catalogue happened to be divided into just six magnitudes we must regard as largely a matter of accident; an accident in the same sense as it is an accident that our foot is just 304.8 mm., and not a little more or a little less. With a more delicate eye Hipparchus might have made twelve magnitudes by making each magnitude half its present compass; and, in fact, he indicated in regard to some stars that they were rather larger or smaller than the average star of the magnitude to which it was assigned by the terms μείζων and 'ελάσσων. The point of interest is to see whether the magnitudes presumably thus of equal compass, forming to the mind a decreasing arithmetical series, will have for the photometric quantities of light emitted by average stars of each magnitude a geometrical series decreasing by a common fractional ratio. If this is found to be true, then the psycho-physic law holds, and astronomers must take it into account.

The first notice of the existence of such a ratio and of its determination that I can find is given by Steinheil (1835). Steinheil's photometer has an object glass divided into two halves, and the light of the two stars to be compared is thrown by prisms, one into one and the other into the other half. Both

stars are put out of focus so as to appear as discs by sliding the half objective towards the eye-piece, and the brighter disc is enlarged until the two are equally bright; whereupon the position of the two half objectives, with reference to the focal distance (by the law of inverse squares), shows the relative reductions of the light. For illustration's sake he chose thirty stars whose estimated magnitudes were known, and he expressed the amounts of light emitted by each in terms of one of them. Arranging these in five classes, he finds that there is a ratio by which the amount of light of a star of any magnitude is to be multiplied in order to equal in brightness a star of the next higher magnitude: that this ratio is tolerably constant, and equals on the average 2.831. Fechner's revision of these observations gives 2.702.

At about the same time Sir John Herschel made a similar comparison of stars at the Cape of Good Hope, but concluded that the quantities of light emitted by stars of various magnitudes formed a series of inverse squares, such as 1, $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{25}$, etc. But Fechner has shown that Herschel's own observations really correspond more accurately to a geometrical progression with the ratio $\frac{1}{2.241}$ than to the series above proposed, the sum of the deviations by least squares being 2.719 for Herschel's series and only 2.2291 for the geometrical series. As Mr. Peirce says, "So powerful is this natural influence [to make equal ratios correspond to equal intervals] that even Sir John Herschel's scale, which was conceived by its author to conform to a very different photometric law, really does conform to this and not to the one he desired to follow."

Johnson, in 1851, compared the light of two stars by reducing the light of the brighter (by diminishing the aperture of the object glass) until it equalled the latter, and found as the ratio of light between two magnitudes 2.358 from sixty stars of from the 4.1 to the 9.7 magnitude. Johnson also deduces the following ratios from the catalogues of previous astronomers, and assigned to each an appropriate weight to mark its reliability. He makes for Herschel 2.46 (wt. 1), for Struve 2.61 (wt. 2), which would be 2.41 if he had taken 13 instead of 12 magnitudes as limit of telescopic vision; for Otto Struve 2.46 (wt. 1), for Argelander 2.32 (wt. 3), for Groombridge 2.58 (wt. 1.5), for his own 2.36 (wt. 4).

Stampfer (1852), from the observation of 132 stars of fourth to tenth magnitude, fixed the ratio as 2.519, and from the observation of small planets 2.545.

[Dawes (1851), by a peculiar and much discredited method, arrived at a ratio of 4.00. This has been so unfavorably criticised, and so many sources of error in it have been pointed out, that it will not be considered here.]

Pogson (1857) compared the light of stars by finding the size of the aperture of the object glass necessary to extinguish their light, and concluded that the ratio (from observations of thirty-six small planets and stars) is 2.427; but proposes as the ratio to be adopted by astronomers 2.572, whose logarithm is just .4.

Seidel, (1861) who used Argelander's estimations of magnitudes and photometrically measured 175 stars with a Steinheil photometer, deduced 2.8606 as the ratio, mainly from determination of the brighter

stars. He mentions, however, that the ratio is subject to many irregularities, and that perhaps it decreases as the stars decrease in brightness. Mr. Peirce deduces 2.754 as Seidel's ratio from stars to the 3.5 magnitude.

Wolff calculates from his observations that the ratio for passing from the 2nd to the 2.5 magnitude is 1.52:1; from 2.5 to 3 is 1.53:1; from 3 to 4 is 1.51:1; but 3.5 to .4; 4 to 4.5 and 4.5 to 5 have smaller ratios, on the average only 1.34:1. This gives for the higher entire magnitudes 2.310, for the lower 1.795.

From Zöllner's observations of forty-two stars (1st to 6th magnitude), the ratio 2.761 was deduced; from 102 stars (2d to 6th magnitude) of the same observer, 2.366.

Dr. Rosen's observations of 100 stars from the 5th to 10th magnitude give, according to Peirce, the ratio 2.339, with an indication of a higher ratio for the brighter stars.

Mr. Peirce, from his own observations, deduces for stars (1.5 to 6.5 magnitude) 2.773, but on throwing out certain stars affected by a constant error 2.449 for stars of 4.5 to 6.5 magnitude. Mr. Peirce gives reasons for believing that the Steinheil photometer is apt to make the ratio in question too large. Steinheil and Seidel, who used this instrument, give by far higher values than other observers, and the determination of the same twenty-seven stars gives for Seidel 2.780, for Zöllner 2.4275.

On the whole, Mr. Peirce prefers to consider the ratio as slightly decreasing with the magnitude, and proposes the formula, log. ρ=0.486—.0162*m*., which empirically satisfies the observations of Seidel, Rosen

and himself. Here ρ is the ratio in question and m the average magnitude.

One sees from these facts (1) that the existence of a ratio by which the quantity of light emitted by a star of one magnitude is to be multiplied to express the light emitted by a star of the next higher magnitude has been questioned by Herschel alone, whose own observations, however, show that he was wrong; (2) that [with the exception of Dawes] the ratio thus found does not differ very considerably from 2.5 in different observers, and (3) that there are many indications that this ratio is not quite constant, but decreases with the magnitude.

Under these circumstances it seemed to the writer well worth while to reinvestigate this ratio throughout the visible scale of star magnitudes from the valuable photometric comparisons which Prof. Pickering (with the assistance of Mr. Searle and Mr. Wendell) has made at the astronomical observatory of Harvard College. (v. Memoirs of that Obs., vol. XIV).

Their method of observing stars was by means of the meridian photometer. The essential parts of this instrument consist in two right angled prisms to reflect the two stars to be compared into the two similar objectives of a horizontal telescope; of a system of adjusting apparatus by which the stars thus observed could be kept in the centre of the field; of a double-image prism of Iceland spar and glass set in the tube near the focus of the objectives, in order to split the emerging pencil from each objective into two, and so adjusted as to make one pencil from one objective coincide with the opposite pencil of the other objective; of an eye-piece through

which the two centrally coinciding pencils pass, in front of which is placed a Nicol with an eye-stop of such an aperture that it will cut off the two outside pencils, allowing only the central one to pass ; of a graduated circle attached to the eye-piece and the Nicol. The pole star was always used as the constant star, and an observation consisted in determining the angle through which the Nicol must be rotated from the point where the two lights are equal to the point where the pole star disappears, the relative brightness of the two stars being measured by the square of the sines of these angles. Adopting the proposition of Pogson, that the logarithm of the ratio of light between two successive magnitudes is .4, it is easy to form a table of photometric magnitudes corresponding (to the nearest tenth) to the angles thus determined.

In all, 4,260 stars of from the first to the sixth magnitude were thus observed in 700 series, including 94,476 separate comparisons. The special sources of error avoided by this method are that one star is seen at a time, and contrast with bright neighboring stars is avoided ; that the combined light of several stars is never mistaken for one ; that errors resulting from the relative position of stars do not occur ; that all stars are observed near the meridian, thus facilitating the correction for atmospheric absorption, and so on. (v. original.)

An important part of the work consists in the comparison between these photometric magnitudes and the eye estimates of former observers, with a discussion of their deviations. It is these tables that have been here used.

By a simple formula with which Prof. Pickering, for

whose aid I desire publicly to record my obligations, has kindly furnished me, these tables can be transformed so as to become directly useful for the present purpose. That is, the eye-estimations of magnitude of the several observers can be compared with the Harvard photometric determinations of the same stars (or equivalent stars), and the ratio which each observer more or less unconsciously used for passing from one magnitude to the next may be deduced. It must not, however, be supposed that these estimations are entirely independent of one another. There was almost an unbroken tradition which, to a greater or less extent, either determined the estimation of the magnitudes themselves or influenced the habit of those who made new estimations.[1]

The resulting deviations between observers are

[1] "In Ptolemy's catalogue of stars, which is supposed to date from Hipparchus, we find the stars ranged in six orders of brightness called magnitudes. The earlier observers not only imitated this method of indicating the brightness from Ptolemy, but also, each of them derived immediately from the study of the Almagest and its comparison with the heavens the habit which determined the limit of brightness between stars which he would assign to different classes. This must, at least, have been the case with Sufi and with Tycho Brahe. Ulugh Beg was, no doubt, influenced by Sufi, as well as by Ptolemy directly, and Hevelius was in the same way influenced by Tycho. It appears that down to about 1840, Bayer's Uranometria enjoyed a high reputation. Argelander showed, however, that its magnitudes were simply extracted from Tycho's catalogue [and from the Almagest in most cases, s. Argelander, De fide Uran. Bayeri, p. 15 (E.)], and he himself proceeded to make a Uranometria Nova. It is to be presumed, therefore, that he endeavored to model his scale of magnitudes upon that of Tycho, although he may have sought to improve upon Tycho's scale by making the intervals between the limits of successive magnitudes such as would seem equal. All observers of stars visible to the naked eye since Argelander have sought to conform to his scale. It is, thus, easy to understand how all the observers have, roughly speaking, the same scale of magnitudes. On the other hand the scale of Sir John Herschel, which was based on common English tradition from Flamsted (who perhaps imitated Hevelius, but was a careless observer of magnitudes), is very different." C. S. Peirce, Harv. Annals, vol. IX., p. 1-7, where is also given an ingenious diagram illustrating the differences between various observers.

many, and are, with regard to the completeness of the survey, the total number of magnitudes used, the fineness to which the estimations were made, and the method of making them.

The tables of Prof. Pickering, which are readily serviceable for my purpose, are those comparing the photometric measurements with the estimations of Ptolemy, Sufi, Struve, J. Herschel, the Uranometria Nova of Argelander, the Durchmusterung of Argelander, Behrmann, Heis, Houzeau, the Uranometria Argentina of Gould, Flammarion, the Bonn observations, (Argelander), and of Prof. Pickering himself. Other of the tables there given are also indirectly useful for this purpose.

The total number of estimations thus furnished is very near twenty thousand, all but eighty-five of which fall between the 1st and the 7th magnitude. The estimations of each observer were distributed in a somewhat peculiar manner, there being always an undue number of stars estimated as being just of the 2d, 3d, etc., magnitude than of the 2d to 3d, 3d to 4th, etc., when that mode of estimating magnitudes was used. The rule followed in condensing tables arranged on this plan was to sub-divide them into divisions in which the even magnitudes came at the centre and the intermediate divisions to either side, dividing the exactly intermediate division into two, and counting half for the group above and half for that below, when necessary. Moreover, the average photometric result corresponding to any one magnitude, or subdivision of a magnitude, was weighted by the number of stars observed as of that magnitude; and the stars of intermediate magnitudes were

weighted by half the sum of the number of stars to either side of the even magnitude with which they were grouped, so as to bring the average estimation at exactly an even magnitude. Where the tables were given in 10ths of magnitudes, both the photometric result and the eye-estimates were weighted by the number of stars observed, and the groups formed by taking all the stars from the middle of one magnitude to the middle of the next, counting the number of just 1.5, 2.5 magnitudes, etc., as half for each. The photometric results corresponding to exactly one magnitude of interval were then calculated from the average weighted 10ths of eye-estimation (which seldom differed much from unity in either direction). With the exception of the eye-estimates of Professor Pickering, which were made with reference to the photometric magnitudes as well as with especial care and with the avoidance of many sources of error (and of a few observations by Sir J. Herschel, which have not been here considered), all the tables show one serious and one more or less decided deviation; they estimate stars of the first magnitude too bright. Or perhaps one ought to say that some of the stars of the first magnitude are so intensely bright that they make the average star of the first magnitude much too bright; or again, that the stars enumerated as of the first magnitude really should be sub-divided into two, those of the first magnitude and those few preëminently bright stars which one might term the 0th magnitude. It is also to be remembered that there are fewer of these stars to be observed, and thus greater room for error. A similar but opposite effect is noticed in the fact

that in the six cases in which basis is given for calculating the ratio of 7th to 6th magnitude this ratio is too small; these six ratios present great discrepancies, and the result is not of great reliability. My method of correcting for these errors is to calculate the curve which the other four ratios follow and calculate the positions at the "2-1"[1] and the "7-6" ratio from the formula thus obtained.

Another peculiar irregularity is to be found in the two ancient catalogues of Ptolemy and Sufi. The ratio from "3-2" to "4-3" undergoes only a very slight fall or in Sufi's case even a rise, but in passing from "4-3" to "5-4" a sudden and most decided fall. I see no ready way of accounting for this except perhaps that these observers may have had in mind a general comprehensive distinction between bright and faint stars, and that in the desire to separate the two they made a gap between the 1-2-3 and the 4-5-6 magnitudes. No such effect occurs at all in the modern catalogues. On the whole, as the importance of these catalogues for this purpose is slight, it seemed better to omit the ratios in question, and perhaps it might be best to omit Ptolemy's and Sufi's catalogues altogether, the effect of which would be to slightly lower the resulting ratios.[2]

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[1]The 2-1 ratio, 7-6 ratio, etc., means the ratio for passing from an average star of the 2nd to an average star of the 1st magnitude; from one of the 7th to one of the 6th, and so on.

[2] It should be added that Houzeau's table gives a value for passing from the 6.7 to the 6th magnitude, which I could not use. Behrmann's ratio for 2-1 from only five stars, and the Bonn observation ratio of 5-6 from twenty-two stars were also not used, for evident reasons.

The general average of all[1] the tables here used gives the following table, including all the above corrections:

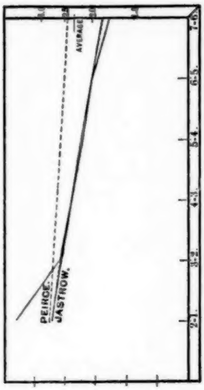
Magnitudes.	2-1	3-2	4-3	5-4	6-5	7-6	Av. of all.
Logarithms of Ratios.	(.5572) Corrected .4474	.4147	.3780	.3360	.3125	(.2501) Corrected .2732	.3603
Ratios........ ..	(3.607) Corrected 2.802	2.598	2.388	2.1675	2.053	(1.779) Corrected 1.876	2.293
Weight, i. e. Number of observations.	288	818.5	1791.5	3746.5	9772	2428.5	

It is evident that the ratio decreases with the magnitude, and the empirical formula thus calculated by the method of least squares that best satisfies these results is log. $\rho. = .49974 - .03486\ m$, or, $\rho. = 3.16\ (.9228)\ m$. Mr. Peirce's results by the same method give log. $\rho. = .48 - .0151\ m$.

The following table shows the divergence between the observed and calculated ratios by my formula. The logarithms are given. It should be said that the magnitude, m, means the mean magnitude, *e. g.*, for passing from 3-2, the value of m is 2.5. As will be seen, the result when we plot the logarithms is a straight line, with a more decided inclination than that of Mr. Peirce. The resulting curve

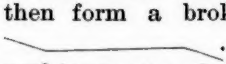
Observed.	Calculated.
.44745	.44745
.4147	.41259
.3780	.37773
.3360	.34287
.3125	.30801
.27315	.27315

[1]The methods of deriving these averages is as follows: After deriving the (logarithm of) the ratio for passing from one magnitude to the next above in the way already described, all these results are grouped with their approximate weights and expressed in logarithms. The weighted average logarithm for each group, "2-1," "3-2," "4-3," etc., is calculated, and these form the uncorrected series of logarithms given in the table. The reasons for plotting the logarithms rather than the ratios themselves is that the logarithms form the simpler mathematical curve, a straight line, for comparison, and in addition obviate the distinction between the ascending ratios and their reciprocals, the descending ratios.



as well as the theoretical curves of Mr. Peirce and myself are given in the figure, q. v.

It should be added that facts favoring this interpretation of Fechner's law exist in other kinds of sensation; but perhaps not in sufficient quantity to allow of a quantitative determination such as is here made. It is universally admitted that the accuracy of Fechner's law (or Weber's law, for from this point of view the two are simply two different modes of experimenting) suffers a deviation both when the stimulus becomes very intense and when it becomes very slight. A more or less extended intermediate region in which the law holds is generally supposed, and the deviations at the extremes, which are admitted to be of opposite natures, would then form a broken line somewhat like this—



. But it is certainly more natural to suppose that the curve is more regular and can be represented by a straight line inclined throughout. Such is the result which the consideration of star magnitude suggests and formulates. Whether and in what way this result is to be taken into account by astronomers must be left to them to decide.[1] To the psychophysicist this method of testing the law is of very especial interest, amounting almost to a new psycho-physic method; even

[1] The astronomers have generally adopted the ratio whose logarith is .4, *i. e.*, 2.512, as proposed by Pogson. The average of the above values is .3603 (or 2.293). If we confine ourselves to stars down to the fifth magnitude the average is .39635 (2.492).

though it be one rough in its nature and limited in its applicability. The psychological processes involved in this kind of experiment differ so much from those employed in the more current experimental methods, that a comparison between the two is extremely valuable ; and is made the more so as it is capable of furnishing the grounds of the validity of the inference from Weber's to Fechner's law.

The general conclusion reached by my investigation is that the law regulating the ratio of light between stars of one magnitude and those of the next above or below it, is the psycho-physic law as formulated by Fechner, with the modification, however, that the ratio in question, instead of being perfectly constant, decreases slightly with the brightness of the star, and may provisionally be regarded as following the empirical formula, log. $\rho = .5102 - .0353\, m.$, where ρ is the ratio of the light of one magnitude to that of the next below it and m is the magnitude intermediate between the two between which the ratio is to hold. All this is claimed for stars down to the sixth or seventh magnitude only ; what the law is for fainter stars remains to be determined.

PSYCHOLOGICAL LITERATURE.

Proceedings of the English Society for Psychical Research. From July, 1882, to May, 1887.

Phantasms of the Living. By EDWARD GURNEY, M. A., FREDERICK W. H. MEYERS, M. A., and FRANK PODMORE. Two Vols. 1886.

The movement out of which the English Society for Psychical Research grew seems to have been Professor Barrett's paper before the British Association in 1876. This was the year in which the experiments of Mr. A. E. Outerbridge became known in Philadelphia, and in which Dr. George M. Beard, of New York, began his publications on muscle-reading. Unlike these American writers, Professor Barrett surmised that muscle-reading was an inadequate explanation of the "willing-game" just then becoming popular in England, especially when the transference of impressions was accomplished without contact, and the older theory of thought-reading seems on the whole to have prevailed. Although the problem termed by Professor Barrett "supersensuous perception," was the vital and chief experimental one, the English Society, at its organization in 1882, had in view the broader object of investigating modern spiritualism generally. The president, in his opening address, declared it "a scandal that disputes, as to the reality of these phenomena, should still be going on," claimed that there was at least a "*prima facie* case for investigation," and warned his hearers that in so strange a field it was only very gradually that all the complicated precautions needed to exclude possible illusion or deception could be learned. Professor Barrett, too, recognized, though not very graciously, Dr. Beard's six sources of errors, and referred to the prejudice with which this subject is usually approached.

Thus far not only the formation of such a society, but the boldness of its plan, with its committees on apparitions and haunted houses, and on the claims of Mesmer and Reichenbach, and the degree to which the difficulties and dangers of the proposed investigation were realized, were all such as to commend it, not only to every psychologist, but to every true and intelligent friend of culture and of religion. While those who regard the baser forms of modern spiritualism as the refined and concentrated embodiment of all the superstitions of a remote and barbaric past, and the claims of those who pretend to mediate between the living and their friends who are dead, as a nameless crime against the most sacred things of the soul, must feel a deep interest in such work, there is another class, perhaps still larger, and with an interest still deeper. This class consists of those who, in these days of unsettlement in religious beliefs, hope to find amidst superabundant *aberglaube* a nucleus of certainty for at least the doctrine of immortality. The most absolute idealists are not so satisfied with the speculative method which works by exhausting thought-possibilities as not to

welcome the most empirical refutation of materialism and mechanism. Even Mr. Meyers's "phantasmagoric efficacy," his "telepathic percolation," or veritable ghosts of those dying or dead, or even in great danger, are not unwarrantable in establishing his "solidarity of life which idealism proclaims," or "the universal mind in which all minds are one." But the impartiality attainable in most fields of scientific research, while it is the ideal to be striven for, in fact is impossible here. A rigorously unbiassed, and yet an intelligent jury, could probably not be found in this country, or in England, so many and subtle and remotely ancestral are the conscious, and far more the unconscious, prepossessions which enter like Schopenhauer's primacy of the will, making us all lynx-eyed to all that favors one side, and bat-eyed for all that favors the other. It is the fact of this inexpugnable bias (which, as has been well said, evolved from a state of savage superstition, so predisposes men that every occasion for it to show itself is utilized, and is therefore dangerous to modern culture and civilization, which enters unconsciously into our judgment regarding all such evidence as seems yet attainable in this field, weakening strong and reinforcing weak proof), that must be constantly kept in mind in seriously striving to form a just critical estimate of the voluminous printed work of the English Society. Even in reiterating the oft-expressed regret that so few men of science, trained in habits of exact observation, to offset the per-fervid and ever-fascinating exuberance of Mr. Meyers, who imposes on his brilliant imagination least of the temperance and suspense bred by the methods of modern science, coöperate in the work of the Society, and especially that trained psychologists and alienists should hold aloof from it, we shall seem to many to express a bias of our own. To these matters we shall recur.

Mr. Creery, a rural English clergyman, had five daughters, who were between the ages of ten and seventeen, in 1882, when the first report was made. Mr. Barrett, who is, we think, a professor of physics, certifies that they were "all thoroughly healthy, and as free as possible from morbid or hysterical symptoms." Their father states that from October, 1880, he had spent "night after night for several months, an hour or two each evening," in experiments on thought-transference with his girls, and seems to have noticed none of the hysterical symptoms, excessive fatigue, dizziness, trancoidal inclination, faintness, *égaré* look, or other pathological effects that alarmed other observers and correspondents of the society who tried similar tests with adolescents. At Easter, 1881, Mr. Barrett was admitted to the family seance. The most meagre account of the conditions of this session are given, and no record was kept of the number of guesses allowed. In August Mr. and Mrs. Sidgwick made a few tests, the conditions of which are not detailed. In November, and again in February of the next year, Prof. Balfour Stewart did the same, and reports his results, stating, however, that "while they cannot stand on the same footing as those of Professor Barrett" and others, they may have corroberative value. In April, 1882, Mr. Meyers and Mr. Gurney tested the girls in sessions covering six consecutive days. In August the three oldest girls went to Cambridge, where experiments were renewed day by day for ten days also by Mr. Meyers and Mr. Gurney, and in December, 1882, Professor Barrett again, alone, renewed tests in Dublin. The percipient power of the children gradually declined during this time, "so that at the end of 1882 they could not do under the easiest con-

ditions what they could do under the most stringent in 1881." This decline, it is later stated in 1886, "had nothing whatever to do with any increased stringency in the precautions adopted."

That the precautions did grow more stringent there is abundant evidence. At first "*all* silently thought the name of the thing selected," after it had been written down and showed to the rest of the family. "The presence of the father seemed decidedly to increase the percentage of successes." At Cambridge, where the three elder sisters were isolated from their family, and where usually none of the sisters but the guesser knew the card selected, the successes were less numerous. Very significant is the series of thirty-two experiments where the sisters knew, and only the tops of their heads were visible to the guesser, and the suit was named correctly fourteen times running, with great positiveness and reiteration, while the card was right but five times. From twenty-seven experiments at Dublin, with the other sister knowing, the committee felt justified in saying that the presence and assistance of the sisters made no appreciable difference in the result, while at Cambridge only eight trials without and seven with the sisters knowing are given as the basis of a similar inference. From these random data the careful reader must infer that the effect of the presence of other members of the family was far from sufficiently studied. Indeed the opposite conclusion from that of the committeee is suggested. The latter expressly ascribed failure under strange conditions to diffidence, and aimed mainly to exclude only *conscious*, and underestimated unconscious collusion, which the long previous practice at home must have made almost inevitable with a set of adolescent girls, however honest or healthy. We should even like a more explicit statement as to how the other sisters were excluded from knowledge of the object selected, where they were, etc.

The methods of isolating the guesser are perhaps still less satisfactory. At first (Easter, 1881) the child was *sent* into the next room, and the name of the object was written and showed around. In April, 1882, the child was recalled by one of the experimenters and movements in the room were excluded after the recall. Later a watcher was sent from the room with the child, and on being recalled the latter was placed with her eyes away from the company. Once at least she was isolated behind the door, at Dublin "behind an opaque curtain" [although we have found an hypnotically sensitized subject who could read large letters in sunlight through five thicknesses of cotton sheeting]. In this most vital respect also there was not only no rigorous control and no systematic method, but only the first rude and irregular preludes and approaches toward it. Such conditions also can serve only to satisfy those present, or with the bias for a pneumatic view of things above referred to.

Again we cannot forbear asking if *every* experiment, without exception, in Mr. Barrett's session of Easter, 1881, to say nothing of Mr. Stewart's sessions, was recorded; and if the results were all noted on the spot and the conditions and descriptions taken at the time and place. The experience of the Seybert commission in these respects, especially the latter, as well as the writer's own experience with the untrustworthiness of memory, even for an hour, where such complex conditions and interests are involved, are sufficient to justify this query. The report of July, 1882, is not explicit on these matters, and, from a careful scrutiny of it, it appears

at least doubtful. When was the paragraph in quotation marks beginning page 21 written, and when the very general description of conditions on page 20? If Mr. Barrett himself made or dictated these notes on the spot, how could he, as the only person present besides the family, possibly so secure himself against all the manifold and subtle sources of fallacy in observation as to be warranted in calling his tests on this occasion "absolutely unexceptionable and conclusive," as he recklessly does. Next in importance to the method of experimenting in so hazardous a field is the way of making the protocol and working up the final form of the report. Either this or the accuracy of observation, or more probably with but one observer, however good, against so many possibilities of error, both must suffer. If either of these surmises is correct, it bears against the statement that the girls gradually lost their power from any cause but increased precautions on the part of the experimenters.

In some of the Creery experiments the precautions of the committee might possibly be impaired by the number-habit, then unkown, and now probably but partially explored, or by corneal reading, or quite broken through by muscle reading without contact of the manifold, and as yet but little known, forms and possibilities which the committee seem to have mainly ignored throughout. We know as yet comparatively little about the constitution and laws of what Dr. Carpenter used to call our automaton. One thing however, is more and more realized, viz.: that it is far more sensitive than our super-liminal conscious sensation, that it is indefinitely more complex and manifold, that no culture has ever approached in its diversity, subtilty or unity the limits of brain possibility in these respects. Neural is far wider than psychic action—the latter involving the former, but not the former the latter. Only when the former reaches a certain intensity, and perhaps extensity, and lasts a certain time, does consciousness, which represents as through a loophole aggregate cerebral states and changes, arise. In the study of hyperæsthesic states we now begin to realize the possibilities of our sensory organism, and how greatly the limits of just observability vary at different times and states and in different persons, and how it responds physically to the remotest and faintest cosmic influences. Hearing, *e. g.*, which is known to vary exceedingly among people whose auditory sensitiveness passes for normal, the writer has carefully tested in many persons. Two individuals were selected for exceptional acuteness in this sense, and the following simple code devised by the writer, which, though repeatedly tried in critical companies, has never been detected, and with results that impressed many as genuinely telepathic. Pulsations, felt subjectively by the percipient, and easily counted by the agent, either by movement of the toe if one leg was crossed on the other, or directly seen in the aorta, or in vibrations of hairs or neck-ribbons, were the basis. The faintest respiratory noises through the nose, or even mouth, of the agent, were made to coincide with and accent certain pulsations of the percipient. To make these modified breathings so distinct as to be clearly heard at a distance by the normal sensitive, yet so faint as to be inaudible even if listened for by spectators often nearer than the agent, was the art of the latter. If a number was selected by some one present the agent caught the rythm of the percipient's pulse, and could hold it for some time if blindfolded, or then see it in the toe occasionally,

and gave a very faint sniff coinciding with a pulsation, and from this, as zero, the percipient counted till the next sniff for the first digit, then till the next sniff from the second digit, and so on. For cards, first the suit and then the card, were counted off. The alphabet went more slowly, but a series of diagrams, from which the selection by the company was made, which had been memorized and numbered, was the greatest success. When the percipient's ears were stopped, coughs, jars on the table or floor, etc., helped us out. This trivial code, however, essentially depended ultimately for the absolute security it generally possessed on the fact that the percipient could hear more acutely than any one present, and when that is the case a telepathy not outside the ordinary channels of sense is possible. We have no data whatever for believing that the ear can hear and distinguish muscle or pencil sounds in making different letters, hear whispers through a couple of doors, etc., but neither have we adequate data for judging how far these, or even less desperate possibilities in the field of vision, would need to be stretched to account for some of the Creery phenomena. Again, a man enters a strange house and wills with the family that a girl in another room bring him certain things in various parts of the house. Would he not almost inevitably in the first session, follow in selecting objects, the suggestions of those who knew what was in the house, and whose mind had been trained both generally from infancy and specifically by the long-continued practices with each other, to concillient action with that of the percipient, and who would moreover not wish her to fail in such an exhibition of her power? When saucer was suggested and written, and plate was brought, the percipient said, saucer "came into my head, but I hesitated, as I thought it unlikely you would name saucer after cup as being too easy," indicating that here, at least, she was controlled by deliberate judgment about the degree of ease or difficulty with which a test was suggested by the preceding one. This suggests clever prediction of the action of minds whose processes are thoroughly familiar—a kind of integrated as opposed to multiplex personality, as in the case of the Siamese and less identified twins—at least so far as the lapse of ordinary associative thought is concerned.

Our chief regret after all is that in the Creery tests the methods were so variable and the results so inadequately worked up and presented, that those whose interest does not centre in the factitious problem of telepathy can get so little aid from them. The poise of a Newton would have even withheld all results till at least some mode or law was either demonstrated or at least disposed of. Is it color or form, suit or card, eye or ear or muscle-sense, that is the preferred medium of transfer? These vital points, towards which we think a truly scientific mind would instinctively tend, are only cursorily mentioned and apparently as afterthoughts. They could best be studied by a careful scrutiny and tabulation of errors which would very likely prove more luminous than successes, and these were not only not printed, but appear to have been only recorded as errors. The color of the card, *e. g.*, ought to be right twice as often as the suit. Were it still more often right it would suggest that transference could be more hopefully sought in the field of color; if one color was chosen most often, that preference would be a fruitful theme for further research, and suggest the kind of precautions, etc. If nine and five are often confounded it would suggest transfer by ear, as in the case of a fleshy and wheezy

agent we heard of whose unconscious laryngial innervation, while thinking of figures and numbers, suggested them to a percipient; if three and two, it might suggest muscle-sense, as the tops of both figures where the hand begins to make them, are alike, so that they are often confounded in muscle-reading, as also are six and eight. If the letters most often mistaken are those shown by Javal's tests to be those with least individuality and most liable to be confused at a distance, in sunlight, etc., we should infer visual transfer. For all such directions of studies, however, or for such data as the experiments made must have afforded had the record been complete and full, we seek in vain. Thus if we conclude, from the very striking results presented, something unknown and independent of consciousness and will, the alternative is by no means necessarily something extra-sensuous or immaterial, or in any degree absolved from the conditions of time and space. So far as this is inferred, we have only another illustration of the inveterate vulgar tendency to associate all unusual manifestations of man's unconscious automatic nature with supernatural powers, whether of good or evil. No special study of such popular chapters of psychology as dreams, witchcraft, hallucination or hypnotism, unless made on the basis of long apprenticeship in experimental biology and physiology and the study and observations of nervous diseases generally, is likely, as it seems to us, to give one that sense of the depth and breadth and number and subtility of physical processes underlying and overreaching and encompassing our conscious psychic activities that is so indispensable just at this point, to prevent an investigation so well begun and representing such high attainment and ability from ending—we will not say as abortively as the other well-known attempts to explain these phenemena made within a century, for it is already far beyond them in methods and results, but without utilizing all the rays of light which modern science now sheds from so many and widely separated points upon the great central questions of psychology. The percipient's descriptions of his mental processes so often adverted to in the reports of the Society are probably of less value than the long account of their symptoms neurotic patients are wont to give physicians who will listen from courtesy and diagnose quite independently of the patient's morbid legend. The degree of reliance on the undoubted good character and *mens conscia sibi recti* of their subjects the committee manifest, despite the irrelevancy of these considerations, which Dr. Geo. Beard has made so plain, and perhaps even the uncalled for allusion to their own veracity and intended accuracy [" The possibility of collusion was excluded unless our own veracity be impeached,"] which are beyond all question, are by no means designed to give hostile criticism an air and sense of personal discourtesy, but are only an irrelevancy expressing the tendency we deprecate.

We have dwelt with some detail on the Creery phenomena because Mr. Gurney, Mr. Meyers and Professor Barrett, the most active workers and probably the best observers in the English Society, spent so much time on them, and because the former expressly states that "it is to those trials that we owe our own convictions of the possibility of genuine thought-transference." Hence a bias certainly existed in all subsequent experiments. The precautions grew more strict, and probably, as we infer, the record grew fuller and more exact, and what is called a decline in telepathic

power we should interpret from between the lines of the record as an approach to the heart of the mystery, which ought to encourage unbiassed investigators to press on toward a beckoning goal. The girls grew discouraged and did not succeed with each other, it is said. This is natural, as interest in their performances diminished. But it is strange that this decline should coincide step by step with closer study of them, and still more so that all the girls should lose this marvelous power *simultaneously!* Never was a momentous discovery wrested from nature with less labor. Groups of agreeable ladies and gentlemen at play sustain each other's flagging interest by the admonition that the sensuous demonstration of the reality of the world of things spiritual and immortal is at hand. "One should not let one's self be discouraged," says Herr Schmoll in the proceedings for May, 1887, "by a little trouble when there is a chance of throwing light on events which, correctly apprehended, may lead us to the psychological proof of our transcendental, imperishable ego." That these investigations have struck the trail of something new and strange, however rare and abnormal it may be, there is ample evidence; but so far they have given us only the opinions of individuals either emboldened or perhaps formed by very exceptional experience in a field of great popular interest and little positive knowledge, attractively narrated some time afterward with illustrative extracts and a few very summary tables from notes taken at the time.

The second group of evidence for thought transference, next in importance to that obtained from the Creerys is that where forms, often not easily described in words, seemed extra-sensuously transmitted. In the first series of these Mr. Blackburn, an editor, was the agent, and Mr. Smith, "a young mesmerist living at Brighton," was the percipient. They had frequently practiced together previous to their first meeting with the committee in December, 1882. The method was as follows: Mr. Smith was blindfolded and seated with his back to the committee, but in the same room with them. Figures were drawn by the latter, and after Mr. Blackburn had looked at them, and then held Smith's hand awhile, he released it, when Smith drew the figure, remaining blindfolded while drawing, as expressly stated in italics. We beg our readers who may be sufficiently interested to try and draw closed lines, bringing the end to exactly the beginning, as is done in fourteen or more cases in these reproductions, with their eyes closed. We have repeatedly tried and failed with each of these forms, whether drawing slowly and irregularly, as in the earlier figures, or smoothly, as in No. 7. Hence we infer either that Smith saw while drawing, which seemed to the committee as likely, and no more so than that he saw while divining the figure, or else that we have here to do with another mild marvel to which they have not called our attention.

About six weeks later experiments for three or four consecutive days were conducted at the rooms of the society in London with the same subjects, and still another series in April, 1883. In all thirty-seven figures were drawn for reproduction, of which fac similes of twenty-two are published. Of these five were with contact. These, however, and even the first series, we may disregard, as it now became with the committee a "cardinal axiom on this subject that no experiment where contact of any sort is allowed can be decisive." For the remaining seventeen the conditions seem more strict than for any tests yet made. The agent was seated and watched contin-

ually in one room, and the drawings were made in another which the committee did not leave. After seeing the picture, Mr. Blackburn, with closed eyes, was led into the room with his sensitive and placed behind him at a distance of some two feet. The percipient sat and drew with or without bandage as he chose, and the reproductions were at once secured. With the record of these seventeen reproductions, without contact, of the most unconventional diagrams we confess ourselves more deeply impressed than with any other work of the society, especially the remarkable No. 22, reproduced with the ears of the percipient stopped with putty, and wraps enveloping the entire upper part of his body. We can but wish, however, there had been more of these, and consecutively, and that while they were about it the committee had isolated the lower part of Mr. Smith's body from all sensation of jars, and carried, rolled or swung the agent into the room, to exclude the possibility of a code by steps which an American exhibitor has brought to an incredible degree of development, and also tested the amount of reduction of acuteness of audition in each ear of Mr. Smith, or at least of themselves, by putty, and taken precautions to make sure that all light from the floor was excluded from Mr. Smith's eyes. We have practiced with some success a toe-code, a part of which is by minimal movements of the great toe within a thin shoe, the latter not moving at all, and a part, for complex figures, involving slight movement of the toe of the shoe, which we should think would only be facilitated by the conditions of No. 22 with overhanging wraps to shield it from the committee. Moreover, it is just these larger general forms and relations of parts without finer details that are best transmitted thus, while the latter, as the committee note, are absent. It seems worse than Mephistophelean —indeed we wish we were freer from the fear that it is so in very truth—to even suggest, in place of tension toward transcendental entities, slight practiced movements of the big toe. We do believe, however, that the mumber of possible keys and codes by which these things can be done is far greater than the committee seem to realize, and even that very subtle forms of deceit are sometimes automatic and quite unconscious in the most worthy people. The first four figures where contact was allowed are certainly much better reproduced than any other four in the series, and in Figure 13 an entire change of the percipient's conception of the model was caused by contact, though unfortunately it is not stated whether Blackburn touched Smith again after he had corrected his conception of the original, and before it was correctly reproduced, or whether the second reproduction coincided with Blackburn's memory of it. Again, in Figure 6, contact suggested a remote reproduction. Suggestive, too, is the circumstance that after contact was excluded Mr. Blackburn explains so many deviations in the copy into greater conformity with the originals by mistakes in his own conception, which he had done in no previous case in this or the earlier series. A cross in one would grow too large in his mind's eye against his will; he had "not precisely remembered" another; forgot the eyes in another; focused on one part only in another, and imagined curves in the opposite direction in another. The tests to account for the mental inversion of objects, which strikes us as just the thing, were only forty-two in number, the result being that eighty-seven per cent. of the answers gave the direction in which a vertical and only thirty-seven per cent. gave that in which

a horizontal arrow pointed. May we add that we have found the same advantage of perpendiculars in the toe-code, on account of the relative difficulty of lateral movement? This is doubtless entirely irrelevant, for it is expressly stated that Smith sees in his mental shrine the image of a white arrow on a dark ground and instantly detected the change when a white arrow on a red ground was substituted for an ink-drawn one. This aside, however, we deem the evidence considerable that after contact was given up either a new and less practiced or more difficult code (conscious or unconscious) was used, or else that the unknown telepathic forces were obliged to find and deepen other lines of least resistance.

Mr. Malcolm Guthrie, well known as a writer on the Spencerian philosophy, is the centre of another important group of thought-transferers, and the percipients are young lady clerks in his drapery establishment in Liverpool. His first tests had been made upon his son. "a nervous and susceptible fair-haired boy of ten," who was at first very successful, but whose efforts made him "feel queer," and who was soon "disposed to ensure success by taking a sly peep at the object." Here he would have stopped had he not learned of the success of the lady clerks, who had practiced by themselves, stimulated by an exhibition of Irving Bishop. The protocol here is admirable, taken on the spot by Mr. Birchall and printed in full, and Mr. Guthrie is very positive in stating that there were a large number of "complete successes" where "the possibility of indication was excluded." The first session of April 4, 1883, consisted of four tests with contact and blindfolded percipient, and one pretty complete success. At the next session, which was without contact, the ladies alone were present. In the frequent sessions in April and May and the following fall, thirteen in number, with from one to over a score of tests each, Mr. Guthrie and Mr. Birchall were generally present with the ladies. The party sat in a semi-circle facing the percipient, and one, or more commonly all present, acted as agents, gazing at an object placed or held in front of them, but back of the blindfolded percipient's head. The first thing that strikes the critical reader is that failures are put down only as such, whether entire sessions as those of May 25, August 30, September 26, or single tests, while the words of the percipient and the conversation with the agent, showing the approximations to correct guesses, are quite fully given in successes, although here again failures would be probably more instructive, or at least as much so, as successes. The tests were mostly visual, and thus, so far from the process being "independent of the recognized channels of sense," as telepathic processes are defined, they are distinctly in the field of vision. Hence, if the vision was not due to normal retinal stimulus, however subtle in degree, the images must be centrally initiated and projected centrifugally outward and downward, which even Mr. Gurney is bold enough to urge, in the face of a strong consensus of neural specialists only for those of the more elaborated and variable sort. The percipient first "sees" light on dark, and next most frequently yellow, the brightest colors, and very general indications of form follow. The first attempt made to study the effect of colors might, if systematized and carried out, have told us whether the ease of perception followed the law of saturation or intensity, but was so badly arranged as to show nothing. This and much else makes us wish to know how the percipients were blindfolded, but we are

only told once for all that it was "effectually." How many thicknesses, of what kind of material and of what colors; how, if at all, they were tested as to their sensitiveness to light, which may be completely absent at first, but slowly regained in wonderful degree, as experiments show, by rest. We are not told the position of the light, or whether there was one or more, nor whether there were polished surfaces capable of reflection, whether access of light from below was permitted. The writer knows a young man who has given attention to the position and use of tiny mirrors, drawn by hairs or invisible threads from the shoe-sole, pants, etc., to enable him to see below a blindfold what was taking place above and back of him, trying even watch-guards and chains, bright buttons and eye-glasses carelessly hanging from his neck. In such tricks ladies might possibly receive even unconscious intimations from reflecting surfaces of stones in their brooches or rings, or indeed any polished surface capable of sending light under a handkerchief about the eyes. There is little indication that Mr. Guthrie is aware of the number and subtility of the sources of error in such experiments as he conducts. We have ourselves conveyed indications of form to a confederate by slight conscious movements of the eyes. This code is a dangerous one, for the attention of all is so prone to fasten on the eyes, but the law of the dominance of contours and the motor elements of perception shows how unconscious and instinctive it is. With contact we have conveyed form by motions so slight as to tax even Goldscheider's limits of extensive sensibility by a grain of sand glued to the finger-tip, and deliberately drawn a figure on the dermal surface of the percipient by motions so slight as to escape detection. A very simple and rapid pressure code for figures, with almost no lateral motion, is worked with a little practice. Another possibility of error lies in the tests with names and letters. If one thinks of a letter and either says or points to each letter in the alphabet, a good muscle-reader divines what letter is in mind by unconscious and unavoidable modifications of finger or voice when it is reached. In the Guthrie tests a free conversation is held. "Has the word come to you?" says the agent to the hesitating percipient, who responds *z*, which is the last phonetic sound it "has." "Right," says the agent; "*i*" at once says the percipient. After guessing *o* rightly and hearing the word right again, now perhaps without any modification of any of its phonic elements, the percipient murmurs *p* and *m*, and when the agent says *No*, at once responds *n*, which is right. As there is internal evidence by indirect quotations and other ways that we do not have a full stenographic record of all the often protracted, conversation leading up to the correct guesser, suggestions unconsciously given and received of this kind are at least not consciously excluded.

In the report of November, 1883, sixteen reproduced drawings selected from one hundred and fifty obtained by Mr. Guthrie and his subjects are published. The original diagrams were "for the most part" made in another room and placed behind the agent, and later in those published, on the agent's side of a wooden stand on a table between him and the percipient, the latter being blindfolded. When the percipient professed herself ready to draw, the picture was concealed and the blindfold removed. Of the sixteen, which seem to have been produced after considerable practice, and with these more strict conditions, contact occurred

in but three cases, which are reproduced better than the rest, certainly if we exclude the first six complete and consecutive tests of a single sitting. With one hundred and fifty ever so partial successes it would seem that some induction could be made as to the parts of the figure or the forms that were best or worst reproduced. In those printed angles are nearly always retraced by the percipient. The same is true of curves, especially circles, with which angles are never confounded, although curves are repeatedly given in the wrong directions. It would seem that vertical were quite well distinguished from oblique or horizontal lines. A straight vertical line is drawn with a crescendo and then a diminuendo of rate, as Vierordt's experiments show, and also of pressure and of noise, as experiments with a spring pen on a large rotating surface made in the psycho-physic rooms of this University showed. Such a line is readily distinguished from a curve by the ear alone. A gradual change of direction involving new sets of muscles, a single movement in one direction, a sudden change in direction, series of repeated movements, large and small lines of the same sort, heavy and light, etc., are not hard to distinguish. A few tests with such a code, as near to nature as we could make it, have at least convinced us of possibilities, and we commend it as a subject for further special research as a kind of psycho-physic auscultation. This does not explain to our mind by any means all in all of the sixteen reproductions of this series, but we should like to know how these exceeding broad and scratchy lines of the originals, which are reproduced indifferently, now in very heavy and now in fine lines, were made. Such a suggestion, however, may after all only serve to divert attention from some entirely different mode of transmission.

In July, 1885, Mr. Guthrie reports further researches, assisted by Professors Lodge and Herdman, but complains of a falling off in the success of his experiments, shown also in his tables. One percipient had been lost, the novelty and vivacity of their seances he said was gone, and he had lost power to give off impressions. Whether the percipient had lost power he does not know. The professors do not appear to have made the precautions much more effective, although they placed the percipient blindfolded facing a corner, and placed objects on a screen at the back of her chair which were seen from behind by the agents. We are not told the position or number of the lights, the nature of the screen, the reflecting quality of walls or floor, what precautions were taken in placing and removing objects. Suppose the screen to be metallic or resonant, or even hard, or the objects handled without care not to hit them against things in a way to produce noise, then we may have suggestions by sound, as in the well-known game of guessing any one of a dozen objects by their sound when struck, which a well-known philologist thinks the primal origin of names of objects. Were precautions taken that no floor shadows of the object should be cast, and has Mr. Guthrie ever tried that other parlor game, once very popular in this country, of holding up objects at a distance of from a few feet to a few inches (according to the sensitiveness of its agent), from the face and neck, to be guessed by their differences of radiant heat? Surprising facility in this latter game we have known to be gradually lost by fatigue or consciousness, as with these subjects. In the ease with which colors are divined, especially if bright, the repeated substitution, in objects not well

known, of contours for surfaces and of surfaces for solids, all suggests, as one of Mr. Guthrie's subjects insists, real vision, and not a mental impression by thought alone. This circumstance, and the continued phenomenon of inversion of right and left, and of reminiscence, or late effect of a previous figure, seem to us very suggestively to invite further special research in each direction, which was not attempted.

Professor Lodge makes an independent report on his "some dozen sittings" with Mr. Guthrie's subjects. Like several physicists with whom we have conversed on this subject he conceives the relation of mind to brain as very likely analogous to that of electric energy to the conductor, as not confined to its contour, but exerting an influence "like a faint echo" in adjacent space, and so perhaps affecting other near brains, but so slightly that they do not commonly notice it. He says that no reliance was placed on or care taken in the bandaging, but he shows, although in a strange field, the training of a man of experimental science by the valuable suggestions of two agents, thinking at the same time of a different object, and again of two percipients and one agent, but neither was fairly tried.

Of the tests made by J. W. and Kate Smith, and by Max Dessoir, both, but especially the latter, are not only poorly reported, but seem to have been made with so inadequate a conception of the sources of error, that, although we are assured that "deception conscious or unconscious is altogether out of the question," the indications are, to our thinking, quite otherwise, and their reports of their accounts do not merit detailed criticism.

The tests made by A. Schmoll, translated in the proceedings of May, 1887, are decidedly more striking, but the eyes of the percipient were very slightly covered, merely, it is said, to make direct vision impossible; real objects were handled, and figures drawn with a match dipped in ink in the room, the time required to divine the object was very long, often fifteen minutes or more; the original drawings were not preserved; it was not even noticed at the time whether a watch, laid on the table to be seen by the agents and divined by the percipient, was going at the time or not (the notes stating that the ticking would be drowned by the noise of carriages in the street, was too far off, etc., but Mr. Meyers states that M. Schmoll had proved afterwards that it was not going at the time); all present generally acted as agents, so that no one was left to observe the percipient. The jar of heavy carriages referred to, while it would obscure sounds, might rattle some of the objects lying on the table, and thus suggest, by audition at least the tea-pot. Of the twenty-six experiments reported, some must be called complete failures, and it is a matter of individual judgment to say how many approach precision, which the experimenter claims for but eight.

The above experiments of visual form and hearing are, as Mr. Gurney says, by far the most important and convenient. The tests with tastes and smells, the latter of which is practically almost inseparable from the former in the case of many substances, were usually with contact; but even where the substances were kept bottled in another room and the hand of the agent applied to the percipient through a sliding trap in the wall, we are not even told by whom or just how the substance was applied to the senory surface of the agent. The experiments of Vintschgau and of Camerer, to

say nothing of Jäger, show such subtility of smell with flavors and aromatics that we need hardly assign more than great sensitiveness, hardly amounting to hyperosmia or hypergeusia, to account for all that is reported in the field of these senses. Pains again are so closely associated, by such subtle reflexes, with motor reactions or tendencies to the same, as was experimentally shown in the well-known demonstrations on the reflex frog in Ludwig's laboratory by Baxt, and, as Mr. Gurney pertinently adds, are readily applicable only to a few widely separated tracts of dermal surfaces, that muscular suggestion is almost inevitable, and we think by no means excluded in any of these tests.

We have thus very hastily reviewed all the leading experimental work of the society. Mr. Gurney states that "from an evidential point of view" the facts are "of an extremely simple kind," and Dr. Morton Prince, of Boston, gravely says that "no physical experiments in the laboratory have been more under the control of the chemist and the physiologist than have these." The simple conditions of experiment are, it is said, to exclude unconscious guidances and contact. The exact opposite is true. The conditions are as infinitely complicated as the psycho-physic constitution of man, and the sources of error are as much more numerous than those in physical science as man is more complex than the substances and forces it studies. What individual can catalogue all the scattered known sources of error, to say nothing of those as yet unknown, in this vast field? Fallacies of observation, of evidence, of language and statement, defects of character and heredity, tricks of our automatic nature, subtle and manifold far beyond all conception, the countless possibilities of illusion conscious and unconscious, so great as to suggest that the boast of the great French magician that he would agree to make any man believe in the normal state that he saw anything, may not have been so very wild; the unfathomable passion for deceit, both conscious and unconscious, that sometimes runs in veins through the natures of men of best reputation and most honest purpose—all these and many more are involved and must be exhausted before telepathy can be positively demonstrated as a residual fact. Hyperaesthesia, too whether normal or morbid, opens up a new world as truly as the microscope or telephone. One tells the form, substance and even color of objects near him by radiant heat, or reads as in a mirror, shadows from walls that seem to others unreflecting; or, in one lately reported case, sees the shadows of heat vibrations over a hot substance cast on a wall by moonlight; the sense of a personal presence is felt by the blow or noise of breathing or heat. The case of Dr. Taguet's patient, who, it was said, was able to read script held behind her head by reflection on a plain card in front of her; the case reported by Dr. Sauvaire, who recognized the suit and number of a card in a different pack, as, it would *seem*, by seeing through it; the case of Rocha's clerk, who seemed to use a piece of card-board as a mirror in which he could see all that took place behind his back; and the well-known case of Bergson's reader of images reflected in the cornea—all these cases are very inadequately considered by Mr. Meyers. If these degrees of hyperæsthesia, normal or even hypnotic, are possible, and were possessed by the subjects with which the English society experimented, the entire experimental basis of telepathy vanishes. Moreover, there is a wide field of unexplored possibility. If blinded bats avoid objects in flying by fine sense of greater barometric pressure

near objects, we may reflect on the possibilities of perception of aerial pressure by highnly sensitized subjects. We have no less good reason to complain of the very inadequate way in which the society has treated the subject of suggestion. We regard the book on this subject by Dr. Ochorowicz as one of the most valuable contributions in this field, as the best statement of the chief rival hopothesis of telepathy, and the one we think every truly scientific man must prefer so far; but the treatment of its contents by Mr. Podmore is very light, illustrating again, in fact, the easy way of ignoring serious difficulties and objections which characterizes the society. Then there are codes and signals, and sometimes quite elaborated languages, by steps, inflections, accents, intervals, rustles and movements of every mobile organ. Thus, not only by the arts of diverting the attention, which, if it is sharpened in one direction, is dulled in all others, but even in the very focus of attention the man of sharper can do what he will to and with the man of duller sense, and seem to work by forces "independent of the recognized channels of sense." Add now the extreme rarity of all those qualities of mind which make a good observer, and the strangeness and perhaps great rarity of the phenomenon, and the probability of error in so hasty conclusions is vast.

Dr. Prince states, as is often implied in the reports, that "no established law is controverted" by the conclusion of telepathy. But the law of "isolated conductivity" formulated fully by Johannes Müller, which Helmholtz compares in importance to the law of gravity, first brought order into the field of neurolgy by insisting that impressions never jump from one fibre to another. If the law be true, an optical impression of the highest intensity may pass along centrepetal retinal fibres within less than a hundredth of a millimetre of an auditory fibre without in the least being able to affect the latter. This law is so generally accepted as fundamental that Gudden states that "in the presence of an anatominal fact, all physiological facts that seem against it loose their significance." Indeed, two severed ends of a fibre cannot be put into so close contact that physiological action can pass from one to another unimpeded. Even those physiologists who admit that certain phenomenon may possibly be explained by the old theory of "sympathies," caused by a stimulus jumping across from one fibre to the next, admit that it is both rare and morbid. The oft-trusted illustration of magnetic induction certainly is not valid here. Is it likely that a neural state should jump from one brain to another, through a great interval, when intense stimuli on one nerve cannot affect another in the closest contact with it. An American essayist at a scientific club lately claimed that all associative processes, by which one idea or impression calls up the sequent state of consciousness, are cases of telepathy within the individual brain. But however long the steps that thought may take in the rythm pulsations by which it advances in brains of looser and coarsely woven tissue, it must now be always assumed to imply uninterrupted contunity of neural texture.

Even the fundamental theory of Bell has to be modified, so far as the brain is concerned, to meet the exigencies of the telepathic hypothesis. In Mr. Gurney's scheme of hallucination, centrifugal projection, or escape downward, may even be from the cortex through the basal ganglia to the peripheral organ. Qualified forms

of projection have been often assumed, but the matter is so complicated and so under dispute, that despite the strong light shed by Kandinsky, of whose chief and latest work Mr. Gurney has not taken account, we cannot see that the centrifugal theory of projection along sensory nerves can be proven, nor is needed. It involves the possible blocking of sensations in the *corona radiata*, does not take account of the fact that strong ideas do not usually excite hallucinations, and that as Galton has shown, great men are not prone to mental images. Impressions upon the senses may take on the psychic quality wherever they will in the passage inward to the cortex, one thing remains probable, viz.: that the more central the origin of impressions the more complex it is, and the more peripheral and sensuous the less attendant concepts there will be, the more the logical connection will be broken through and the less sense of inner activity there will be. More attention should have been bestowed to this point, with all the above tests and subjects. So far, however, as the phenomena are described or can be inferred, they indicate the same field of vision as real things, and suggest nothing akin to imperative ideas, projected sensations of central origin, rather than any subjectively created, or critically evolved sense of objectivity.

Very instructive is the experimental investigation of Mr. S. J. Davey on the errors of observation. He was some years ago nearly convinced of the truth of spiritualism by some sad and strange experiences, but was happily saved therefrom by learning and becoming very expert in a few tricks, especially that of slate writing. Assuming a professional name he gave seances to many intelligent people, requesting them to write down exactly what they saw. Many of these descriptions are published in a very long article in the proceedings for May, 1887. The sitters "never detected the *modus operandi*," and their conjectures about it are ingeniously diverse, and illustrate in many cases a strong propensity to a miraculous explanation. But the strangest thing of all is that we are not told how the trick was done, so that we have no point of departure from which to measure the amount of errors with each guesser. Whether it be that the love of mystification is stronger than the love of science with Mr. Davey, or whether he is under obligation of secrecy, which he does not even deem it necessary to state, his attitude is yet that of a conjuror pleased with his trick and the sense of human gullability it gives. A scientific man states the method by which he got his results; not so Mr. Davey. The society professes to desire to protect men from the common delusion in this field. In our judgment nothing whatever does this so effectively as explaining to them the method by which a few common effective illusions are produced. The acquisition of power to do these tricks it is easy to see was what saved Mr. Davey himself from the abyss of spiritualism, against which it is the most potent prophylactic. The trick-books do not retail the kind of illusion here involved, the conjuring business, if it is so desirable to save it, would not be injured materially. Spiritualists will persist, at least, till details are explained, that Mr. Davey is mistaken in thinking he used only natural means. It is almost impossible to exhaust the various methods by which some of the leading tricks are or may be done, but a good collection of descriptions of methods by which a few tricks most closely connected with the phenomena of spiritualism are done would, we think, in the end be the most effective of all disillusioning agencies.

Again, the theory of probabilities is perhaps the most instructive part of the modern logic of science, but the use made of it in these reports we regard as utterly misleading. Mr. Gurney even goes so far as to state that "the argument for thought transference cannot be expressed here in figures, as it requires 167 nines; that is, its probability is far more than the ninth power of a trillion to one." Has he forgotten the obvious truth stated by Mr. Edgeworth, in the first of his valuable papers, that the calculus of probabilities "is silent as to the nature of the agency, whether it is more likely to be vulgar illusion or extraordinary law." "This," he adds, "is a question to be decided not by formula of figures, but by general philosophy and common sense." M. Sorel well says that it is indispensable to consult experience frequently to know if the phenomenon can be sufficiently approximated to the ideal play of chance, as even games of so-called chance are not just applications of the theory of probabilities, though commonly thought to be. In the face of this commonplace we are obliged to say that the way in which appeal is so often made to this theory is the only thing in the work of the society which seems to us lacking in ingeniousness. This aside, however, there are other interesting incidents in these researches that shed light on the general applicability of this theory. Everything runs in groups. There is the Creery, the Guthrie, the Schmoll group, and, as Mr. Guthrie says, "the good averages run in lots," and he thinks that the calculus of probabilities does not help conviction, adding that one successful évening, when the conditions are good and the truthfulness of the percipient genuine and simple, is a better guarantee than any subsequent cross-examination of results. A friend of the writer missed in guessing the numbers of a die the first thirty-seven times, and if there had been as much interest in finding errors as successes, the former may have been as strangely grouped and bunched. As in games of chance, and in the records of gambling houses, there would seem to be as great individual marvels of bad as of good luck, did not the former always tend to be eliminated. In fact we have spent much time and labor in repeating with many subjects, nearly all the experiments of the English society, only to find in very many cases an unaccountable proportion of error. In many of these tests, at least, conditions are not known—not controlled—and the numerical basis needful for a fair average is not established, so that we do not know what "absolute chance" means in a given case, or what was the original *krasis* of things, what is the average error, or how errors are grouped. There is a sense, too, in which the probabilities against any given occurrence are infinite.

These points need fuller treatment, but we must hasten on to note the fallacious conception of evidence in such a field. Much is said about "spreading responsibility," the "cumulative" nature of the proof for telepathy, increasing the number of people who are knaves or idiots if it is not true, and the multiplication of instances is compared to increasing the size of a bundle of faggots, each one of which is easily broken, till together their evidential value is irresistible, and, last of all, prizes are offered for good tests, etc. In a word the society's conception of proof is quantitative. This is an imposing argument in America. When we are told that seven million children are following the Union Sunday-school lesson course, when enthusiastic spiritualists claim still more than that number of believers in their doctrines in this country as proof of pedagogic or

doctrinal merit, we reply that evidence is to be weighed, not measured by bulk. Quality of proof should be chiefly regarded in such matters as psychic research, and not quantity. If one-half the people in the world accepted telepathy and the other half rejected it, it would by no means follow that the probabilities pro and con were even. The cumulative method has the advantage of encouraging the bias above referred to both by mutual countenance and evidential appearances, but in science it is the competent minority that is usually right and the majority that is usually wrong. One man who would exhaust all the resources of modern science in precautions in this field, would be more authoritative than all the parlor sceances together. What is to be chiefly desiderated is not the multiplication of instances, but more systematic and prolonged study of such cases as have been already found, the use of more cautions against error, the probability of which would be shown so incalculably great could the calculus be intelligently applied to their estimation.

Next to the fundamental assumption of telepathy in this class of cases we regard as the capital error of the society the association of the above so-called "experimental basis" with that class of phenomena illustrated by the seven hundred and two weird tales in the Phantasms of the Living, or with "spontaneous telepathy" at a distance. Mr. Gurney frankly admits here "a certain gap" or "incompleteness in our transition, which must be admitted without reserve," and yet elsewhere says it is impossible to tell whether he would have credited the validity of telepathy in the spontaneous phenomena had they not been confirmed by the "experimental basis." In the latter cases will and attention were involved to such an extent as to effect the robust health of Mr. Guthrie, and in the former cases consciousness is less involved. Mesmerism at a distance brings in other factors too abnormal to really constitute a transitional case. While spontaneous cases seem to occur at different distances, from a few feet to thousands of miles, no serious tests of the effect of time, or even distance, strange as it may seem, were made in the experimental cases. Mr. Creery thought that his daughters were most successful at the distance of a yard or two, and a few very inconclusive tests as to the effect of distance were made upon Mr. Smith, but there appears to be no reason to infer any experimental results save at very small distances, (if suggestion and trance is excluded), while for these distances the time seems to vary, with no suggestion or search for a cause, from an instant to fifteen minutes or more. To us the most natural and obvious inference, which is certainly not excluded, seems to be that the two series of cases are due to entirely different causes, no more related than are coincidence and collusion. Again the experimental results were chiefly in the field of the higher senses, involving special forms, and are matters of utter emotional indifference, while the spontaneous cases, which, indeed, sometimes touching the nadir of triviality, as in ghosts of clothes, warm water for shaving, etc., are mostly such as profoundly involve the affections, like death and danger of friends. The collection of tales is of the greatest value, and it is significant that it is the last moment of this life and not the first of another that seems to have most of Mr. Meyers' "telurgy." But we believe the final verdict of science respecting them will be that they illustrate the great mythopoetic tendency by which fancy unconsciously grows into similitude with fact, just as

organisms adapt themselves to their environment, a tendency that is rather to be sought below the threshold than "above the upper horizon of consciousness," as Mr. Meyers believes. The only psychological explanation we can suggest for the premature and almost passionate identification of the experimental and spontaneous cases as telepathic is the constraint of the potent but secret bias in favor of "superconscious" states, of a "soul-politic," or perhaps even "molecular meta-organisms," and in general toward "the solidarity of life, that realism proclaims." But this is surely the idealism of a Swedenborg, and not that of Plato or Hegel.

It illustrates, in contemporary form, Kant's "Dreams of a visionary explained by the dreams of a metaphysician." Our experience, in fact, is not unlike that of Kant, who, after paying a great price for the chief work of Swedenborg, and spending much time in its perusal, concluded, in substance, in his well-known article of the above title, that such pneumatological conceptions were pseudo ideas, formed by the negation of sense, made thought free from not *in* the world, and were idola illustrating the *morbid* tendency of some minds to come to a focus outside of themselves, and that for his part he would henceforth turn his back resolutely from the seductions of such considerations.

When we reflect how few are the well established facts that are exact and certain, and on the labor by which they were demonstrated, or on how rare are well ordered cohesions of thought or the associations that approach anything like real mental continuty, and on the inestimable educational value of these in making possible even a limited area of well woven mental tissue, and remember that modern science is already the greatest achievement of the human race, to bring one solid contribution, to which individuals are more and more content to spend a life of labor, we are reminded of Kant's well-known simile of an island surrounded by an unknown and very tempting, but foggy, stormy sea. In this sense telepathy is of the sea and not of the land. It is, on the whole, much less removed from modern spiritualism than from true science, so far as all telepathic theories go. Spiritualism, in its more vulgar form, is the sewerage of all the superstitions of the past. Wherever there has been civilization and culture, it is because its dark clouds have lifted for a space. It is the common enemy of science and true religion. It has led astray many able men. The beginning of science and philosophy has always been doubt of its claims. The majority of men, living and dead, are its adherents. It is against its claims that skepticism has its leading justification. To clear up its dismal jungles, and drain its unwholesome marshes, is probably the work of centuries. In modern biology, culminating in neurology, where so many of both the secrets and the revelations of science are coming to centre, that one might almost say the undevout neurologist is mad, a firm foothold is at length secured in which mind and matter, so long and so widely divorced that from the fallow between them wild and unsightly growths had waxed strong, and thick, and old, have a common interest, and the dangerous chasm between them is slowly closing. Physicians appeal to the imagination in desperate cases with bread pills and placebos, are less ashamed of interest in hypnotism and are less disposed to regard even hysteria as the *summum incognitum*, and the study of insanity as worthy of the briefest of all courses in medical schools, while clergymen and metaphysi-

cians, on the other hand, who used to practice healing arts in the good old time, when "Godlike was the doctor, who was also a philosopher," are beginning to take some interest in the body, and to read books on mind cures, and psycho-physics, hygiene and physiological psychology, and to realize that the student of religion and of idealism cannot, with impunity, neglect the study of the common forms of morbid psychosis. We desire, for our part, to see the psychological movement, which now seems destined to mark the present as the psychological, as the last quarter of a century has been the biologico-evolutionary age, kept in the severest sense, experimental and scientific. The dangers and difficulties are vast, and the specious false ways many, but we have a nucleus of solidly established facts, and the reward of every achievement is likely to be at least no less than any that have crowned the progress of science in the past. But we must ever remind ourselves that while "strange things are true, they are not truly known till they are related to what is tested, else they remain solitary and unfruitful."

Great credit is due the English society for calling attention afresh to the mysterious side of human life, and for later making known to English readers something of the valuable work of the French investigators of Paris, Nancy, etc. Mr. Meyers has taken great pains to see many of these men and their work. If good hypnotic subjects are more numerous in France than in England, it would seem that ghost seers are most common among cultivated classes in England. It is to be hoped, however, that the indication of more independent work in the study of abnormal states now apparent will lead to more solid results, and that the crude and premature theory of telepathy, which is by no means impossible, *per se* in some sense, but as yet lacks everything approaching proof save to amateurs and speculative psychologists will be allowed to lapse to forgetfulness. To the careful and patient experimenters and observers in this field there are now far better and far surer and far more useful results than these, though by methods far harder and slower. But it is by these that we prefer to labor.

Psychology. The Cognitive Powers. By JAMES MCCOSH, D. D., LL. D., etc., President of Princeton College. New York, 1886. Pp. 245.

Introduction to Psychological Theory. By BORDEN P. BOWNE, Professor of Philosophy in Boston University. New York, 1887. Pp. 329.

Psychology. By JOHN DEWEY, Ph. D., Assistant Professor of Philosophy in Michigan University. New York, 1887. Pp. 427.

The work first on the above list is to be supplemented by another on the motive powers of the mind, including conscience, emotions and will. The cognitive powers are here treated in three books as respectively presentative, representative and comparative. Dr. McCosh has taught psychology for thirty-four years, and compares his work to Uncle Toby's stockings, darned till hardly a thread of the original fabric remains. The book is neither dull or dry, but abounds in apt quotations in prose and poetry, stories, illustrations, sudden and unexpected but always impressive morals and hortatory passages, and seems to reflect, in the clearest and most direct way, the strong and beneficent personality of the author, not only

his convictions, but even very many incidents from his own experience being interspersed. Almost every page contains taking points admirably presented to catch the wandering attention of listless students in non-elective classes. The book is of value to every thoughtful teacher of this subject for its pedagogical suggestiveness. It is evidently made up of three factors: General matters of miscellaneous sorts, which, in an unusually prolonged experience as a teacher, its author has found effective and beneficial with the average college senior; the essential points in the Scotch philosophy, or more particularly in Thomas Brown, Stewart, Butler, Macintosh, Abercrombie, A. Smith, etc., which have survived from a long-ago study of these writers; and, thirdly, such material in contemporary psychology as in some cases its commanding importance has brought to the attention of every eminent administrative educator, and in other cases such as mere accidental or personal relations (as with his distinguished pupils, Professors Macloskie, Allen Starr and F. M. Baldwin), have impressed upon the author's mind. That with his advanced years, his heavy educational cares and responsibilities so vigorously borne, and his early absorption in the Scotch philosophy, the limitations of which those who most directly inherit its traditions now best see, Dr. McCosh should have maintained a mind so open to so many of the newer influences in the rapidly widening field of psychology, is a striking illustration of the beneficent effects of the true spirit inbred by studies in this domain, and makes the task of the honest and friendly critic particularly unpleasant. Judged from a scientific standpoint, however, little that is good can be said of the book. The wood-cuts of brain and sense organs that are inserted are but little more related to the text than the marginal figures with which ancient missals were illuminated were wont to be. It is perhaps something to associate the study of perception in the old abstract fashion with even the pictures of these things, although but in the most casual way, as we associate a book with the tree under which we read it. There is an apparent incommensurability between seeing, feeling and thinking on the one hand, and the visual and tactile image of the corona, corpora and vermicelli of the convolutions, on the other, to the novice, that even mere juxtaposition may alleviate. Symbolic figures like the onden of Mr. Betts or the pyramid of Dr. Hopkins, or the circles of modern logicians, or current diagrams illustrating aphasia, etc., have obvious illustrative value. The relation between thought and brain, however, is anything but obvious. but appears more plainly as the anatomy of brain and analysis of psychic processes become finer. It is far less, and perhaps not all by virtue of its morphology, but rather by virtue of its finer anatomical and chemical properties, that the brain is the organ of psychic activities, as yet but imperfectly unknown. This, we believe, should be carefully indicated, or else the anatomical part passed over, in elementary teaching. Many of the allusions to finer structures and processes by Dr. McCosh are inexcusably careless, to use no stronger terms. We are told that "all along the spinal column there is automatic action which is reflex." "There is a cell called a ganglion into which one nerve enters and from which another goes out." Questions of structure are referred to physiology. The communication from the spinal cord is "up by the medulla oblongata and the crura cerebri to the corpora striata and optic thalami." "The action to the brain travels at the rate of 140 to 150

feet in the second. The action from the brain travels about 100 feet in the second." The author hastens on through this strange region, which is dismissed with a caution that all materialistic ideas must be left behind, despite the temptation of youth to the contrary in the study of psychology. "We are not to allow ourselves to look on mind itself, or any of its operations, as occupying space, as extended or having figure, as having weight or levity, height or depth, elevation or depression, attraction or repulsion, solidity or elasticity, motion or rest, light or darkness, warmth or frigidity." Even words derived from material objects, as idea, psychic, spirit, feeling, emotion, impression, understanding, conception and apprehension, must be stripped of materialistic associations with their etymologies. But why then the anatomical illustrations, which not only precede, but follow? Why then the skin with its "two layers," and the nerves in the tongue, fingers and lips "generated at these points by use," and "the muscle sense, including in it the volition and the resistance which first gives us the idea of Power, Potency, Energy or Force, out of which proceeds our idea and conviction as to causation?" Why are we told that "distinctness of vision requires that objects shall be so far apart that their images on the retina shall reach more than one cone?" Why, apart from the many such inaccurate or mistaken statements, is space given to the anatomical and physiological relations of aphasia, memory and association, etc.? Still we are thankful for the good will towards scientific psychology, and commend the sagacity that sees its importance, even if the former be as yet all unreconciled with the traditions of the intuitive school, and the latter uninstructed in details.

A still more grave defect of the book is the essential failure of the author to profit from both Greek and German philosophy. There is abundant evidence here, and in his other works, that he has never taken the trouble to acquaint himself, in any historic or sympathetic way, with the great writers in his field in both these languages. He elsewhere declares that idealism has no place in philosophy, and that the latter will never be properly established till this is acknowledged, but pleads for the old Scotch "realism," as the ideal "American philosophy." As the Scotch school may be said to represent hard-headed common sense, without the refinements or subtleties that are bred of specialized research, by any set method or direction, this is a most convenient attitude for a busy man, who must keep up the semblance of philosophy on short allowance of time and information, and must commend itself to many practical American minds who cultivate the power to make summary snap-judgments on all topics, finite or infinite. We believe, however, that blindness to the great lessons of historical philosophy involves the gravest loss to students. A course in idealism, as treated by Kant, whom our author cannot abide, Plato Hegel and the rest, we believe, stimulates the development of mental power, gives inner resources against all corroding pessimisms, tact to solve the practical problems of life and mind and zest, breadth and insight in any intellectual career unsurpassed if not unequaled by any other element of modern education. It especially illuminates religious sentiments, and gives both poise and a repertory of weapons against doubt, and ought to be entirely indispensable to all who would speak and be heard on religious

topics. That Dr. McCosh, with his great and long opportunities, has failed to utilize these deep sources of wisdom, we regard as deplorable for the real interests of religion, as well as of science. This, we believe, will be the verdict of those laborers in the philosophic field most nearly in sympathy with the religious standpoint of the author.

Once more there is often a dogmatism and self-assertation which is only calculated to entail prejudices and seriously to limit the unfoldment of mental power and future effectiveness. After stating that man's knowledge "begins not with relations, but with with things," he adds, "in laying down this proposition, I undermine one of the most fatal—as I regard it—errors of the day." After saying that the infinite is both beyond our widest thought, and that to which nothing can be added or subtracted, he says: "After working out this two-fold aspect, I found that I had been anticipated by Aristotle." The great problem whether we are conscious of all our mental operations, is dismissed with the statement, "I hold that we were conscious of the acts at the time, but that they were not retained, as there was nothing to fix them in the memory." Again, "I do not agree with the theory of those who ascribe the creations of genius to unconscious mental action." Each of these is a commonplace view long current in philosophical literature, but is stated dogmatically and in the most momentous manner, without facts or arguments to sustain it, as if it were a great and original discovery. Thus he concludes "we have traced the powers of intelligence from the lowest to the highest, and have shown how our cognition and ideas arise." This modest claim is hardly calculated to encourage further study in this field. The book abounds in irrelevancies and discontinuity, and is of all grades of merit, from the extremes of garrulity to very impressive hortatory perorations. Had it been clearly recognized that the problem was to write an attractive primer in psychology, bringing together only the results most universally assented to, and of most practical importance, and pedagogically first, the book, with some material and many minor changes, might have been made commendable. Teachers who introduce youug men, seniors though they be, into these studies, must expend their wisdom in showing where to begin, and shunning the inculcation of a sense of finality, furnish incentive to those who need it to pursue their studies further in the theological school, the psycho-physic laboratory, or graduate historico-philosophical or educational study. This book illustrates, in a word, not realism in any saving sense as the author claims, but ecclecticism in every respect, which makes that word philosophically offensive.

Professor Bowne's book is mainly devoted to what he holds to be the underlying principles of pure or introspective as distinct from and presupposed by all forms of empirical psychology. These principles, he thinks, are best illustrated in common facts, and that an "authology of madhouse and hospital stories" has an "odor of quackery." Though physiology "means well," and is an "estimable science," its influence in reconstructing psychology seems to the author declining. He is conscious that in his book many "will not find what they want," and "still more will find what they do not want," and many arbitrary omissions are confessed, owing to the plan of the work, but others are as free not to read as he to publish, etc. The work falls into two parts—the factors of the mental

life, and their combination. The starting point is the analysis of the individual consciousness. Psychology is a subjective and not an objective science, and is based on introspection. It is not truly studied by an analysis of language. Psychogenesis, observations of animals, etc., "admit of almost no experiment," and its "facts admit of no exact measurement." "The man who feels cold is cold," etc. All materialistic assumptions are to be "repudiated in advance." Anatomical discreetness is inconsistent with mental unity. If the brain secreted thought we could collect and look at it as we do bile. Materialism rejects the reality of the self as the subject of the mental states, which is the burden of what positive doctrine the book contains. "Thought and feelings demand a subject, and have no meaning apart from it." "Rational life, by its very nature, demands a unitary consciousness and a unitary subject." Neither the matter of the physicist, nor the thinking matter of the hylozoist, nor the theory of two parallel series, is rational. "If materialism be true reason is exploded." It is depressing, has no standard of truth, afflicts the pure psychologist with "tedious superficialities and drolleries." "What ever progress brain physiology may make it will never bring us one step nearer to materialism." It has "an irresistible tendency toward error, superstition and falsehood," and it has "falsified experience at the start," and gives a "manikin conception of humanity." The difficulty in identifying physical and mental facts lies in their complete unlikeness. Vibrations are not sensations. "No peering, even into the living brain, would give the least suspicion of the mental series attending it." Again, nerves never feel. Sensations are mental reactions against nervous actions, and are not passed along "from one atom to another, like a letter from hand to hand." A sentient nervous action is a square circle. The doctrine of the specific energy of nerves "has been largely abandoned." It is the "terminal structure" in which the specific energy resides. Thus "concerning the particular form of the nervous action nothing can be known," but "our complete ignorance of what takes place in the nerves is no psychological loss." Neither practically nor "psychologically should we be better off if we knew all about the form of the nervous action in any special experience and the place of its location." All such facts are "not properly psychological facts at all," nor even "facts of any kind" to the idealist. The psycho-physic law represents "no significant principle." A blind enthusiasm has magnified Fechner's formula into undue importance. "In the name of a mathematical formula, psychology is loaded down with meaningless absurdity." All explanations of after images are "purely hypothetical." The mixture of colors by rotating disk "does not take place in the mind but in the nerves." Such works as Helmholtz—"Sensations of Tone" and "Physiological Optics"—"reveal no new psychological principles." There are probably no unconscious sensations. Ideas have no intensity and also no attractive or repulsive forces by which they separate or unite. The studies of association-time merely show what was known before, viz.: that familiar processes are quickest. The "cerebral theory" of memory, which fills a long appendix, "has generally been regarded as demanding separate cells for the preservation of distinct experiences." Each idea, "we are told," is based on the action of a separate cell. Molasses *e. g.* has an odor, taste, a name for ear and eye is of many kinds and associated with many things, and is after all but one word,

while a man like Mezzofanti spoke fluently thirty, and knew something of seventy-two languages. Each one of all these variations demands a cell, and thus if the cerebralists were right the cells "would get filled up," and the possibilities of experience and knowledge would be exhausted. The facts of aphasia on the cerebral theory "lead to the most fantastic and grotesque assumptions and whimsies." It is all "physiological mythology born of materialism." It "necessarily increases our difficulties without adding any insight," "explains the obscure by the obscurer," abounds in "unmanageable features," is a purely gratuitous hypothesis," a piece of "physiological metaphysics," "immensely increases our difficulties without adding any insight," etc., etc.

The "thought-factor," according to Professor Bowne, works over sensation under the idea of time, space, cause, etc. Sensation is set over against the self, classified and related. If Mill's "psychic chemistry" theory of the origin of space-perception were true, it would "bring thinking to an end." The notion that sensation or that the mind is extended is also a "whimsey." If the thought of extension is extended the thought of infinite extension must require an infinitely large mind to contain it. Mill's view of the nature of the thinking self is "plain nonsense." By the theory of the "permanent possibilities of sensation" "language has been outraged," and "we are in the lowest depths of unintelligibility." "The metaphysical denial of the reality of substance leads to nonsense in the mental world and to nihilism and solipsism in the outer world." "The associative theory is one of the sorriest efforts of speculation." "Materialism cannot be joined with any sensational philosophy without mutual destruction." This alliance is "one of the many inconsistencies of evolutionary thinking." Mind-stuff and psychoplasm are "highly elegant conceptions" as "figures of speech that defy all interpretation." "Evolution has no such importance for psychology as its friends imagine." Its facts are "without theoretical significance." Herbert's deduction of feelings is "a failure in all respects." Physiological æsthetics is rejected, for a noise hurts a nerve no more than a note does. The claim that the self is made out of the sum of mental states is made up of "some extravagance, some ambiguity and considerable nonsense." Fichte's view of the rise of self-consciousness "is an abuse of language." Whether we can be conscious of more than one thing at a time is "an idle question." The view that memory "is the form of mental action most dependent on physical conditions" is "probably much exaggerated." Many facts of aphasia are "utterly opaque on any theory." The treatment of the judgment in formal logic is "entirely false to its psychological character," "highly artificial," and "often does violence to the psychological fact," "a barren study of verbal permutations." This tendency reaches its climax in the later forms of symbolic logic by becoming purely mechanical. The fourth dimension theories are like reasoning on the assumption of a square circle. The soul is in direct interaction with the brain, but need not be in it, but at an infinite distance from it, and in fact is not in space at all. The subject of localization of the functions of the brain is "in entire uncertainty." That the ground of insanity is physical "can hardly be said to be made out." Yet the soul and body are in some kind of interaction and mutual dependence. "Certain forms of memory seem even conditioned by physical participation."

Besides these salient points, the book of Prof. Bowne contains much current psychological matter and a few subtle criticisms. Though his spirit is much more narrow and provincial, the author is far better read in both the ideal and empirical literature of his topic than the writer of the book noticed above. But his work surpasses anything we have ever read in the field of modern psychology, not only in its hardihood of brunt denial of accepted facts and interpretations, which if sustained would reduce many a settled consensus back to the plane of debate, but in offensive and ill-bred language, which can only tend to lower the tone of the controversy, and which fills us all along with painful doubts whether a self-respecting reviewer ought to touch it. Students, whose knowledge of psychology was derived from this book alone, would be led to believe that all workers in a vast field of science, not only deal largely in "plain nonsense," "whimsies," that "outrage language," "are loaded with meaningless inconsistencies," if indulged in are liable to "explode reason," "bring thinking to an end," etc., but that scientific men at heart know better, and are "ever seeking to evade," "explain away," "escape" some great and obvious first truth of reason. They would think that those who seriously study the localization of functions in the brain, psycho-physics, symbolic logic, neurological physiology, comparative psychology, psycho-genesis, the two great works of Helmboltz, and all who labor in those fields ; that morbid psychology, the unconscious in all its forms, and everything that savors of matter, evolution or sensation, represent a vast incoming tide of perversity, whipped up, to be sure, by diabolic cunning into fine and insidious intellectual sillabub, which is sweet to the palate, but which it is not merely folly but morally infectious to imbibe. The resources against these new men and methods and topics are first bravado of negation. Have not several critical inventories of human powers shown that understanding can never know this, and reason can never do that? No faculty or investigator must be allowed to poach beyond the lines laid down by the great Kantean survey, even for an hypothesis or conjecture. It is the function of the philosopher to enforce the licet and non-licet of the code. Secondly, mind must be dematerialized, which now means deneuralized. To do this at every point is Professor Bowne's chief effort. Among the many phobias, or morbid fears, now quite well defined, is mysophobia, or fear of dirt, first described in 1878, which impels the patient to wash every object he must touch, and to wash the hands after dreaded contact with everything more palpable than thin air, often scores of times a day, to avoid pollution or contamination. Its analogue we may call hylephobia, or morbid fear of materialism, also a very modern distemper, which afflicts, now and then, a philosopher with a horror of contact with the fresh facts of science so necessary to his survival in the world of modern thought, and impels him to try to purge every element of matter from facts he cannot escape. Hylephobia, however, is now often regarded as a sacred madness, as epilepsy used to be. It befalls only the good ; and the richer and fairer the world of sense, and the more violent the phobia against it, the more surpassingly rich and fair and real must the purely subjective, rational, ideal world appear. All the wisdom of scientific psychology melted in this author's crucible is but slag and dross, and that of so malodorous a kind that not only is he as excusable for the oft-repeated errors and ignorance of de-

tails his pages betray as he would be for holding his nostrils in a foul air, but we suspect that this ignorance and audacious defiance of authorities is a part of the disease, and thus as sublime as the filth in which white-souled anchorites gloried. Thus it would be not only a long, but an all too-thankless, and even idle task, to point out the blunders in detail. Although students of the book would find it infectious of this mania, they would get very little knowledge of the adversary against whom they were to crusade. Indeed, they would hardly suspect even the existence of a vast and concillient body of facts concerning the validity and significance of which there is no dispute among those competent to judge, and still less would they glimpse their vast variety, their wide-reaching suggestiveness, or realize the unsurpassed mental discipline and moral vigor they afford, the quickening of all the psychological roots of the religious sentiments of reverence, subordination and hopefulness they bring. Against the old materialism of Büchner, Moleschott, Carl Vogt, or Czolbe, which is the real object of many of our authors' attacks, and of which many residua still linger, especially among young men, his weapons are occasionally effective, but the psycho-physics of to-day is far nearer the standpoint of Kant than of these writers, and admits, as fully as Professor Bowne himself, the utter incommensurability that appears between a physical solid and conscious activity. He repudiates mad-house tales, but Mr. Galton says: "No professor of metaphysics, or psychology, or religion, can claim to know the elements of what he teaches, unless he is acquainted with the ordinary phenomena of idiocy, madness and epilepsy. He must study the manifestations of disease and congenital folly, as well as those of society and high intellect." The spirit animating this volume is utterly unlike that of Lotze, whom the author followed with such fidelity in an earlier work, or that of Prof. Alexander, who admirably says: "There are two common mistakes—one, the denunciation of physiological methods by men who have never seen a ganglion cell; the other, the denunciation of subjective methods by men who have never given an hour to introspection. It does not appear to be necessary, however, that a knowledge of one set of facts should be incompatible with a knowledge of the other set. A combination of the two is the ideal psychology." We would not lay aside this almost purely negative book, which it is generally very hard to treat seriously, however, without expressing some real obligations to the author, to whose vigorous analysis we are indebted for some insight, and who has pointed out a few real defects in both the methods and inferences of modern psychology. These defects are by no means fatal, but very slight, incidental, and easily corrected. "Indeed," he says, "if our mental possession should suddenly shrink to what we know, the residue would be paltry and pitiable in the extreme. It is only by venturing beyond knowledge that a social or even mental existence becomes possible." This cheap opinion of knowledge may perhaps account for his uncermonious way of treating it, and his struggles beyond it, if it be a struggle for mental existence, every evolutionist will easily excuse. Again, he exclaims in a collapsing or despairing way, near the end of the book, "there is a great body of facts which suggest that the mental life cannot go on without the physical. Can any light be thrown on this question?" That is, indeed, the serious question, but does it not belong at the beginning of any helpful

book, devoted so largely to just this question, rather than at the end? That is, at least, precisely where the psycho-physics he so perhorresces begins, and that is just the question. Even the few isolated facts he reports, if sympathetically scrutinized, start us so hopefully, at least, towards answering.

Dr. Dewey's book is to Hegel as Prof. Bowne's is to Lotze. In each case the spirit of the masters animates the pupil, but has not gained in insight or breadth of view. Dr. Dewey is a less servile disciple of a better master, is on the whole better trained, not only in psychology, but in the general field of philosophy, and his book is pervaded by an indefinitely better spirit, and his material is wrought together with far more vigor, coherence and originality. There is no trace of cynicism or vulgarity. The author unfolds, with the most charming and unreserved frankness and enthusiasm, the scheme of absolute idealism in a simple yet comprehensive way, well calculated to impress beginners in philosophy, to whom the book is addressed, and with helpful pedagogic diversions. Psychology is the science of the "self," which has the power of recognizing itself as I, knows that it exists, or "exists for itself." This is consciousness which "can be neither defined or described." "The fact of the existence of self or of consciousness is accordingly a unique, individual fact." The content of knowledge is universal, for all could know it. Psychology is defined as "the science of the reproduction of some universal content or existence, whether of knowledge or action in the form of individual, unsharable consciousness." Thus "physiological psychology cannot aid psychology directly. The mere knowledge of all the functions of the brain and nerves does not help the science, except so far as it occasions a more penetrating, psychological analysis, and thus supplements the deficiencies of introspection." Physiological facts are "of no avail, for they tell us only about certain objective processes." "The ultimate appeal is to self-consciousness." Knowledge is thus universal, while feeling is individual, and will connects the two. These three are not faculties, but inseperable aspects of consciousness, resulting from artificial analysis, but for convenience made the basis of the three-fold division of the book, the greater part of which is given to knowledge. Here, too, lies its chief merit and originality. Sensation is "the elementary consciousness which arises from the reaction of the soul upon a nervous impulse, conducted to the brain from the affection of some sensory nerve-ending by a physical stimulus." The latter is always some form of motion. "A sensation is a consciousness; it not only exists, but it exists for the self." Yet we are told on the next page that we have no more direct knowledge of it than of an atom, and that it is not immediately present in consciousness. Sensations tell us nothing but their own existence, or how the subject is affected. Motion and sensation have nothing in common. Despite the usual dualistic "chasm," motion is merely a mental phenomenon. The nervous change is not cause, but stimulus or occasion on which the soul develops sensation. A sensation is "the transitions of the physical into the psychical." On this whole topic of sensation, it is impossible to grasp the author's meaning. Sensations are not knowledge. They are purely subjective, separate and distinct, each from each ; in short, chaotic. Knowledge consists in the processes of relating these individual feelings and discrete fragments. They

must be transformed not only into unities higher than those of time and space, objects, relations and ideals, but they must be changed into the self that knows and idealizes. To this end the mind must react upon sensuous material in attention, and retain the appercieved content in memory. Thus sense becomes significant, and its elements coherently related. Association "never leaves sensuous elements isolated." It combines air-pulses to tones, makes all colors out of the three elementary sensations, fuzes and redintegrates according to the familiar rubrics of successive, simultaneous, contiguous and similar, etc. Artists use philosophers notice, the associative tie that broadens but does not burden the mind, and controls habit. These products of synthesis may be disassociated by different influences, as interest or value is given to different elements. Sensations are thus distinguished by tone, by nearness of relation to self, morality, etc., till apperruptive organs, or "ways in which we tend to interpret sensations," are established. Disassociation thus breaks up the mechanism—bursts the bonds that would tie the mind down to objective data, allows it to play freely, according to its interests, and breaks up control by environment. Thus ideal internal ends may be pursued by attention, which is internally initiated, to the ends of the self. Attention is "that activity of the self which connects all elements presented to it into one whole, with reference to their ideal significance." On the fundamental principle that "nothing can be in consciousness which consciousness does not put there," attention, as the organ of selection, is very important. It selects only those elements which point beyond themselves. Thus only interpreted sensations, and never sensations as such, enter into our knowledge. This is idealization, for it passes beyond present existence. By attention the whole organized self is brought to bear or "read into" selected sense elements so as to give them meaning by "reading itself into them." Thus unity, idealization, meaning, distinctness arise. Attention is fundamentally a "self-developing activity." Thus with the aid of the assimulative function of retention "the world becomes objectified self, and the self subjectified world." "The world known is the externalized self; the self-existing is the known or internalized world." Leaving the *activities* of knowledge, its *stages* are studied as perception, memory, imagination, thinking and intuition respectively. *Perceiving* is "opposed to thinking," because it is objective and not subjective. Visual and tactual space are briefly considered, to show how it is the will which separates objects from itself. This is the central distinction in this field where differentiation predominates over identification. *Memory* is higher for the present is transcended. All its objects are "wholly ideal." Past and present are related or unified in rythm. Memory is possible only where there is a permanent self amid changing expressions. *Imagination* embodies ideas and is freed from the limitations of memory. It is a "universalizing activity," releasing the ideal from the petty and particular, making poetry in a sense truer than history, and implies a basal unity between man and man, and man and nature; in short, demonstrates the "universal self of humanity" in organic unity with nature. *Thinking* still further "dissolves out" the universal and ideal "to discover the meaning of facts universally." It is distinguished as (*a*) conception, which "is the apperception of the apperceptive process;" (*b*) judgment, which refers the

ideal, or universal, to the particular element; and (c) reasoning, which is the recognition of relations. The highest reasoning is philosophy, which is "complete science," and seeks to find a true universe. *Intuition* is immediate knowledge of the world, self and God. Every fact is seen to be related to every other, the whole is found in the part, and this completed interdependence is necessity. The world is known because we idealize it, and the self is known because it is realized. This process goes on through the self and from this fact we gain the conception of freedom. God is the true self-related, or the organic union of the self, and the world, of the ideal and the real. The goal of all knowledge or truth is "the complete manifestation of the unifying and distinguishing activities of the intelligence," and all error or agnosticism is emphasizing one to the exclusion of the other of these processes.

Feeling is "the internal aspect of mental life," and exists so far as consciousness is unobjectified. As the latter is never complete feeling, though unique and unsharable is "as wide as the whole realm of self," and is the undividual side of its activity. If the self is furthered, pleasure; if hindered, pain results. Successful adjustment is pleasant. Feelings are sensuous and formal, qualitative, intellectual, æsthetic and personal. The last three have gradually unfolded into universality. Under personal feelings peace, dependence, faith, obligation, remorse, humility, sympathy, love, conscience, etc., are treated. Conscience, *e. g.*, is a "feeling of the universal and objective worth of personal acts, but in what degree the feelings are true to fact depend upon how universal and objective is the self which feels." Will originates in sensuous impulses. It is the self realizing itself. The essence of self is the self-determining activity of the will, which is objectifying activity. Science is the objectified will. Will finds its motive in feelin its result in knowledge. It unites the individual and the universe, joins the finite self and the infinite personality in which truth, happiness and righteousness are united in one.

Dr. Dewey's book is admirably adapted to reproduction by a resumé of salient points and ever recurrent phrases. Its merit and originality are great, but they all lie in the scheme rudely outlined above. That the absolute idealism of Hegel could be so cleverly adapted to be "read into" such a range of facts, new and old, is indeed a surprise as great as when geology and zoology are ingeniously subjected to the rubrics of the six days of creation. The older geneses, whether of the world or of mind, are so simple and ultimate, have been rounded to such epic completeness and sublimity, that as they are superseded by still larger and loftier conceptions, their dissolutive phases are often pathetic. The pathos here lies in the naive unconsciousness with which the system of universal consciousness unfolds all its vast canvas of definition on the stormiest of all seas that science tries to navigate. Definitions make the fibre of the book, and even the favorite form of sentence. The author is always working from partial to complete definitions or conversely. There are scores of formally quite novel definitions of nearly all the subject matter of psychology. They are treated as self-luminous, or, at most, their fit or self-relation is their justification, and these constitute the warp of the entire fabric. Viewed from the standpoint of facts, very few of them are satisfactory, and many we believe to be fundamentally wrong and misleading. To enter upon this, however, could only at most open perhaps long

but certainly fruitless controversy. But the author is more intent on the mutual interpretation and coherence of his network of definitions than on their relation to facts, and it is just this that makes his book as unitary as Dr. McCosh's is rambling and incoherent, as positive as Professor Bowne is negative. The "self," *e. g.*, is treated as something of settled and exact connotation, simple and undefinable and immaterial, without a hint or suspicion of the vast problems opened by both disease and by hypnotism, pointing to its derivitive, or at least exceedingly complex nature. Memory is treated only as a member of a hierarchy of faculties, and with no word to suggest that there now lies the chief field of controversy in psychology between a material and pneumatic view of soul. The whole vast field of what was at first and so crudely termed by Hartman the unconscious, and where the scientific study of psychic activities has of late won its chief triumphs, is substantially ignored, although consciousness itself, with which the author is solely concerned, we are told "can be neither defined or described." To say that an act is unconscious means simply that "the act is done by the body" as a result of simultaneous association.

Besides definitions, the other ingredient of the book is illustrative facts. In the selection and use of these, for which the writer is often indebted to the results of modern scientific methods and is duly grateful, lies the other chief merit of the book, which, however, by a man of great ability as Dr. Dewey clearly is, might have been written half a century ago, and have been poorer only by a number of pat physiological illustrations. The facts are never allowed to speak out plainly for themselves or left to silence, but are always "read into" the system which is far more important than they. They are nearer to the sphere of sensation, incohate, dark, solitary, than to the pure self-luminous light of self-consciousness, which is turned on them in these pages. In the field of these facts the statements are extremely often vague, inexact and even mistaken, and abound in the errors, often petty, sometimes grave, of non-expertness. These we can only sample. "A *wave length* of .00009 millimetre," it is said, can excite the sense of hearing. The retinal image is "interrupted by the blind spot." Flavor is said to involve tactile elements. The tone of a tuning fork is simple; "all others are complex." The whole statement of this great discovery, which Helmholtz calls "the most important of recent times," is vague and general to the verge of utter unintelligibility. Four or five times in the book we are told of the lower and upper limits of tone-perception, and the sensation above 40,000 vibrations a second is repeatedly described as "whirring," a term it hardly seems as if one who had once felt it could apply. "Whirring" is near the lower limit. "It is highly probable that the auditory nerve continues the sound stimulus in vibrating form." Heat is said to be a stimulus that "affects all sensory organs alike." Touch "is distributed by means of the skin over the whole body." Again, "the skin is regarded as made up of myriads of sensory circles." All but hot and cold spots on the skin are said to be "sensitive to no kind of temperature distinction," and cocoine anaethesia and leaves the parts affected "as sensitive to differences of heat and cold as ever." "The reason that we do not see the stars in the daytime is that they do not give $\frac{1}{100}$ of the light of the sun." The psycho-physic law unquestionably merits far fuller treatment in any psychology. Almost nothing is said of in-

stinct or of morbid or anthropological psychology. Omissions, however, may be pardoned, inaccuracies never. If we are to have facts and results of laborious scientific work, let them be stated clearly and exactly. Dr. Dewey's method is through and through speculative, and psychology in its leading features is to him one of the most complete and finished sciences, instead of being in the most interesting stage of uncertainty and incompleteness. Not only all actual but all possible future facts are certain to take their place in this idealistic scheme. They may indeed enrich it, but can never essentially change it. In the open field of research, however, it is precisely these general views that are now most uncertain and wavering. Is self-consciousness inscrutible, and ultimate, and supreme? What is it, what is the self, and what is knowledge? Is there a "chasm?" Is sensation pure and manifold, or is it the most perfect knowledge, reason being sensation in the making, as Mach assumes? What are ideas, and can we know an "organic unity" more complete than, say, a gaglion cell? Is not such an unity rather in the nervous system than in conscious thought? What if consciousness be not only a partial and fragmentary manifestation of individual life, but, as some postulate, a form of disintegration, a set of signs of the imperfect working of our infinitely complicated automatic apparatus? None of these are open questions for Dr. Dewey. It is not enough to know even if we know truly, but we must know that we know. It is not sufficient for light to shine; it must light itself. Even "the perceived world is more than the existent world." One who philosophizes by this method might exactly as well write a text book on any science whatever as on psychology. The light is always essentially lighting itself, from whatever objects it happens to be reflected. As an artist is less interested in the subject of a picture printed on the programme, or the philologist cares less for the story of a classic writer, but both are more intent on an ulterior analysis that shall reveal the great elements of style and motive, and reach a meaning below the author's consciousness, so the modern psychologist studies the great systems of philosophic thought—this with the rest. In the system of "progressive self-realization" in the idealistic sense he sees the lift and expansion of adolescent, altruistic forces, always inspiring and ennobling, which every young man is the stronger and broader for having felt, the enthusiasm of which no student of any philosophic subject can miss without grave loss, and to the meaning of which, having felt, he will always remain pious. But it is a stage of development which minds that come to full scientific maturity are certain to transcend. Its phrases grow dim and unreal, and have a hollow, uncertain sound, in the quest of something more definite and real and systematic. Were this issue reached at the end, or tendencies to this larger view seen in the author, the propadeutic virtue of the book would be greatly enhanced. To students inclined to immerse themselves in an ideal view of the world it will prove very stimulating, but dire will be the disappointment of those who hope to find in it the methods or results of modern scientific psychology. The literary references at the end of the chapters will prove very helpful, but those of most scientific value are not much utilized in the text, and nearly all these authors would not agree with the argument, for such it is, of the work. Finally, for classroom use the book is far from satisfactory. Statistics now before us, embracing nearly three hundred colleges, are very far from sus-

taining the statement of the preface that "it is the custom of our colleges to make psychology the path by which to enter the fields of philosophy."

Elements of Physiological Psychology. A Treatise of the Activities and Nature of the Mind, from the Physical and Experimental Point of View. By George T. Ladd, Professor of Philosophy in Yale University. New York, 1887. pp. 696.

Thanks to Professor Ladd's book—it is at last possible to read a plain statement of the facts of a good part of the field of experimental psychology in English. Its merit in this fundamental respect is incomparably greater than any one book in our language, and it is likely to be for a long time indispensable to every student of the subject not familiar with German. Roughly speaking, over five of his nine hundred pages are devoted to a condensed and generally clearly arranged account of results of special scientific investigations, less concisely stated than in Hermann, but more lucid than in Wundt. The facts are often gathered with great industry from many special monographs more recent than the chief German text books, and along some lines brought down to date without substantial omissions. The author is not intent on illustrating any theory or system belonging to an utterly different attitude, period and method, or stage of development, but the system consists in a plain grouping of the facts which are allowed to speak out for themselves. Taken all in all, the book cannot fail to have a most wholesome and stimulating effect on the study of mental phenomenon in the institutions of higher education in this country. It should be read by students of medicine and theology, as well as of philosophy, and teachers who desire to know the scientific basis of modern methods of pedagogy will derive great benefit from its pages. The vast fields of morbid and also of anthropological psychology, psycho-genesis and instinct, which might be included in the title, are excluded, and even within the limits imposed on himself by the author, there are many deficiencies, but from the fact of so large a book, covering only a part of its field, the reader will readily infer the immense accumulation of material which already crowds the psycho-physic domain, and superficial or disparaging text-books in this field will henceforth be impossible, or at least ignored. All this applies to the first two parts, or to the first two-thirds of the book only. The first part is devoted to the nervous mechanism. The nervous elements are first considered chemically and histologically and physiologically, and then their combination into a system involving a sketch of the general anatomy of the cerebro-spinal system. Nerves as conductors, automatic and reflex functions and organs, the development of the nervous system and the mechanical theory of its action, are each given a chapter. Part second is on the correlations of the nervous mechanism and the mind. Two long chapters are given to localization, and two to the quality of sensations, one to their quantity; then come two chapters vaguely entitled the presentations of sense, devoted to the perception, as it is more commonly termed of space, form, motion, etc.; then come physiological time, feelings, and a final and isolated anthropological chapter on certain statistical relations of the body and mental phenomena. These chapters are illustrated by one hundred and fourteen wood cuts, about ninety of

which are anatomical, mostly gross, and copious references to special literature are given the form of foot notes, and there are many tables.

In the present condition of foreign literature, it is far easier than it would have been a few years ago, to compile a book like this; but it must still have required not only much industry, but considerable time. For one not practically familiar with laboratory methods in physiological chemistry, histology, physiology and psychology, to have done this work on the whole so creditably, even from the standpoint of specialists in these fields, suggests the possibility or a division of labor between writers of general treatises and those engaged in experimental research, which may perhaps be helpful to both, and to the cause of science. With so large a book, so filled with facts it is impossible to deal in detail. We shall content ourselves in pointing out a few of the significant defects of the book, which it is hoped may be remedied in another well-revised edition. To dissect out the axis cylinder of nerves to be chemically analyzed by itself is said to be "by no means always easy." All such preliminary anatomy is of course at present as absolutely impossible, as it is indispensable for specific results of pertinence. That this cannot be done renders much of the general chemical information presented scarcely more relative to psychology than to general biology. A compilation of the inferences now indirectly suggested in the field of micro-chemistry, by the action of different staining fluids, would have been better. Again we are told that "one of the processes of each cell may, as a rule, be regarded as continuous with the axis cylinder of a nerve fibre." This general view, which also conditions much else, is rendered improbable, if not false, by the work of both Golgi and of Forel, the total ignoring of which, as well as of so much valuable Italian work generally, is a grave defect. The treatment of cells is inadequate and apparently uninformed, and yet cytological conceptions now seem likely to be those upon which the whole mechanical theory may be reconstructed and transformed. The account of Wundt's theory of the mechanism of nerves seems derived entirely from the chapter in his psychology, which is called "more complete" than his larger work on the subject, but which is very meagre, almost to the verge of unintelligibility, as are the accounts of the theories of DuBois-Reymond and Hermann, and none of the later phases of the question are presented. The chapter on the mechanical theory is probably the most inadequate in the book. On the basis of what is given in this vast topic everyone would agree with the author's conclusion, that a "confession of ignorance might fitly close the entire discussion." In the chapter on reflexes Eckhardt's larger work, which might have been followed, if not as literally as Foster and Balfour confessedly are in the embryological chapter, at least no less implicitly than Hermann's Handbuch is in some parts of other chapters, and other well-known works elsewhere, is apparently unknown. Had it been utilized the chapter might have been materially improved, and especially the work of the Ludwig school on this important topic would have been given due place. Those whose interest in physiological psychology is rooted in metaphysics, attach great importance to studies on the localization of cerebral functions, a line of research upon which many physiologists look with suspicion as a field with which the methods of experimentation in vogue, in the

backward state of psychological analysis, are not yet able to cope in a way to give assured results. The latter are few and briefly stated, the controversies voluminous. Instead of the two long chapters devoted to this topic the utility of a book of this size would be increased by a briefer statement of the results of experiment, with reproductions of the charts of Exner and of Flechsig, which, strangely enough, with all mention of the extremely significant and perhaps epoch-making work of Steiner, are omitted. These three land-marks in this field should by all means be given a place in another edition; but all this material should be more briefly and concisely stated, and more space given to the great topic of aphasia. In such a book Jäger's theory is surely worthy a passing mention. The chapter on the quantity of sensation is hardly less inadequate than that on the mechanical theory, and needs much reconstruction. Not only are extremely significant views entirely ommitted, like that originated by Mr. C. S. Pierce, but Fechner's general conception of the subject, and Delboeuf's method and its motivation are substantially wanting, and there seems to be here, as occasionally through the book, padding with matter, the bearings of which are not fully seen, or at least not stated. The two chapters on perception will prove very convenient in the class-room. In this vast field, also, the work of a compiler must be hard, and great range for individual preference should be allowed. The work of Le Conte Stevens certainly merits mention, and the author has given too little attention to the views of Hering. There is ample field here for more cuts, even if at the expense of some of the common brain chart. The time chapter, though much less useful for students than the later conspectus of Professor James, is valuable. The work done here is limited, and admits of easy statement. Time sense and reaction-time, however, have so little to do with each other that it would have been better to separate the two and to have given the former topic a fuller treatment. The last three chapters of part second are less concise and more speculative, but still very convenient to the student for the facts they contain. As a whole, the work bears somewhat the same relation to the field it covers as President Porter's *Human Intellect* does to psychology from its standpoint. Professor Ladd's work, however, is incomparably harder and is far better done. That everything pertaining to "insanity, delerium, aphasia, somnambulism, ecstacy, mind-reading, spiritualism, and even sleep and dreaming," are "definitely excluded," because these are not normal states, illustrates the extreme superficiality of the demarcation of the field. This has, however, the great convenience, that in these excluded fields lie just the problems which at present seem most inconsistent with the author's appended theory of the reality of a thinking ego, and the final and absolute nature of self-consciousness in the sense held to by him. Over and over again we are reminded of the utter disparity between brain action and concommitant consciousness. Yet diagrams of even the gross anatomy of the nervous centres profusely given, seem to him more relevant than are the above manifestations of the unconscious to the standpoint of the "forever undefinable consciousness." Moreover, just appreciation of the facts in this excluded field is precisely what tends to break down the middle wall of partition between nervous and mental changes, every chink in which metaphysicians are so intent on stopping, and gives a sense of a correlation between them which is something more and nearer

than telepathic. The general disregard of methods of research, the description of which affords a natural, logical approach to experimental results, aids again in isolating neuro and psycho-physic facts.

The experience of a man who has once had the invaluable training of abandoning himself to long experimental research on some very special problem—whether with a nerve-muscle preparation of a frog, the structure of a ganglion cell, physiological time or any topic wisely chosen, and whether working under the guidance of a great teacher or of his own instinct, is often somewhat as follows. At first there is a sense of limitation, a fear of loss in focusing all energy upon so small and seemingly insignificant a subject. It seems not liberal, still less broad philosophic culture. Especially by those used to the unrestrained freedom and slightly enervated by the sense of "vastation" which makes metaphysical speculation so intoxicating, it is often abandoned as less attractive than the free treatment of results of others' work. If the work is pushed on, however, the student finds that he must know in a more minute and practical way than before—a way that comes to make previous knowledge in the field seem unreal—certain definite points in electricity, chemistry, mechanics, etc., and these are brought into fruitful suggestive unitary relations with each other. The history of previous views pertaining to the topic are studied and understood as never before, and broader biological relations are gradually seen. As the work goes on for months and perhaps years it gathers momentum until gradually many of the mysteries of the universe seem to him to centre in his problem. In the presence of one minute fact of nature he has passed from the attitude of Peter Bell, of whom the poet says, "a cowslip by the river's brim, a yellow cowslip, was to him, and it was nothing more," to the standpoint of the seer who plucked a flower from the crannied wall and realized that could he but understand what it was root and all, and all in all, he would "know what God and man is." Even though he may not have contributed a tiny brick to the great temple of science in the shape of discovery, he has felt the *omne tutit punctum* of nature's organic unity in a sense far deeper than speculation knows, and has realized what higher education in the modern, as distinct from the mediæval sense, is. In a word, nearly all the defects in the book before us spring from the circumstance that the facts of physiological psychology, which we are told in the Introduction cannot be called "even a definite branch of the science of psychology in general," are viewed with the eyes of Peter Bell, which seeing, see not. This appears in the frequent dependence on current hand-books, in the perspective with which certain groups of facts are presented, the purely external classification, in a few novel translations of technical terms, as *e. g.*, *Nervenstrecke*, *Zwangsbewegungen*, *Erregungsherde*, etc.

The third part need not detain us. Its merit, standpoint and style are so different that those interested in the first two parts will care little for the third, and *vice versa*. The author's creed respecting the powers and unity and development of the mind and its relations to the brain lead up to the culminating thesis of "The Mind as a Real Being," the failure of experimental studies up to date to demonstrate which occasions their repeated designation of "psychology without a soul." In the preface we are told that "one result of all our subsequent investigations will be to show us that consciousness

and its primary phenomena can never be defined." Histology gives much information, but it is "mingled with a still larger amount of conjecture and doubt." Why brain changes and conscious experience are related "will always remain an unsolvable inquiry." We are "indefinitely far" from "even a reasonable prospect" of a physical science of the nervous system. Science is "not yet able to deal with the phenomena of nervous action, as shown even by a single living nerve with a muscle attached, when acted on by any one form of external stimuli; how much less," etc. It is all "a leap in the dark;" "difficulties are absolutely insurmountable;" were brain-changes known they could not be conceived as a "true cause" of anything psychic. The mind is "a real unit being," "of non-material nature, and acts and develops according to laws of its own" and its "origin and destiny, its mortality and corruptability, physiological psychology finds itself unable to demonstrate, though it may suggest, and perhaps confirm," the author's theories in this field. It may, however, clear away "barriers of ignorance and prejudice," and open up a broader way "to rational psychology, to ethics, to metaphysics and to theology." We shall not quarrel with all this. It can be said in any field of science that there is a something quintessential not yet explored. Science is not ontology, but phenomenology, and there is nothing in physiological psychology to disprove the author's creed nor our own. As to the whole "two clock theory," it is as true in the entire psycho-physic field as Hughlings Jackson well says it is in morbid psychology, that even to understand the brain we must not take too materialistic a view of the mind. The concomitance theory is far better, even as a working hypothesis, than the theory of identity. To say, he asserts, that ideas produce movement, the mind influence the body, etc., implies disbelief in the doctrine of the conseveration of energy, and the use of all such terms as ideo-motor, physiology of mind and even psycho-physics is a logical cross division. If an histologist can use any or even all conscious activities as means to study brain structure, or if structure be studied as a means to a knowledge of function, the argument of correlation vs. ideality is as irrelevant in kind, at least, as it would be in the field of any other comparative study. That the author utilizes his "sense of chasm" by restating so well on the whole, though with such needless prolixity, these inveterate and to our thinking, mild and commonplace Lotzeanisms apart by themselves, instead of having them "read into" the experimental details like Dr. Dewey, erects to our mind another prejudice in favor of his dualistic attitude.

In the whole field of biology, including psychology, it is as often necessary (and probably increasingly more so) to proceed from the complex, whether the organic unity of the cell, the physical individual or his consciousness, to the simple, as from the simple to the complex. That any one who has carefully studied the modern concepts of physics concerning matter and force, however firmly grounded in the mechanical theory, will fail to feel that, as Mach has so fully pointed out, physics may be very soon led to a very different, psycho-sensory definition and conception of matter and force, seems to us unlikely. Striking and grateful as such a conversion would be to those whose whole psychology centres in the theory of knowledge, it would only modify the form of a single symbol, or be a light of different hue shed upon a familiar sense.

It would only aid us to see that conceptions of mind and of matter, of self-consciousness and motion, cannot possibly be disparate and incommensurable, because both are concepts, and equally ideal—that least immediately known, in fact, the most ideal. For theological minds it is consciously or unconsciously the question of immortality that animates all this kind of argument. From this standpoint nothing could be more unwise than to give sensations and in a sense even feelings over to neurophysics as hardly less foreign to the soul as a real being, whose chief function is to unfold *knowledge* by relating these, than body. If this is all the spiritual unity, reality, etc., that remains, and if even its traits cannot be deduced in any a priori way, but must be laboriously sought from an inductive study of particulars, then both the ends striven for in the old anti-materialistic crusade have become even more barren than we thought. Feeling, sensation, the unconscious elements in which most psychic secrets are wrapped up, as well as all matter, force and mind, transcendental and real objects, creed and fact, as well as knowledge and the ego that knows,—not one or a few, but all of these, must, of course, forever be parts or aspects of that complex concept we designate as the world, and hence ideal. For every scientific object, or for every end of knowledge *per se*, if that is the highest, it does not make one hair white or black, whether we work by this hypothesis or by that of realism. The problem between the two, though one of the most seductive of all modern puzzles, is probably the most barren and incapable of solution. The main thing is that we really work on whatever theory. Let those who prefer absorption in self-consciousness really interpret the conscious ego, and be no longer to vaunt the ancient triumph, or repeat the old stages of the method. Experiment is now checked at many points till the work of subjective analysis be done over again, and better and finer. Let those that prefer literary work now take the next step and prepare manuals as full and large as Professor Ladd's on each of the special great chapters in psychology, *e. g.*, psycho-genesis, space-perception, psycho-physic law, physiological time, physiological optics, and acoustics expression, etc., on some of which themes we are glad to know comprehensive special treatises are now well under way. Experiment has already opened, and is just beginning to work one of the richest of all of scientific mines, and every writer and every explorer of subjective consciousness must now know something practically of its methods. Coöperation here already promises, not only important acquisition to the knowledge of man, but the better academic standing of the philosophic departments in our higher institutions of learning. If there is anything on which men may now differ in opinion, and yet abate not one iota of sympathy and mutual appreciation, it is on these points.

All these suggestions aside, however, we desire to recognize, in the most candid way, the great debt of gratitude which all students and teachers in this field owe to Professor Ladd. His work is sure to give a great impulse to those studies which have been sadly hindered by the want of what he here supplies. Even for the book as a whole, we have five parts of hearty commendation for every one of criticism and dissent.

Logical Machines.

In the "Voyage to Laputa" there is a description of a machine for evolving science automatically. "By this contrivance, the most ignorant person, at a reasonable charge, and with little bodily labor, might write books in philosophy, poetry, politics, laws, mathematics, and theology, without the least assistance from genius or study." The intention is to ridicule the Organon of Aristotle and the Organon of Bacon, by showing the absurdity of supposing that any "instrument" can do the work of the mind. Yet the logical machines of Jevons and Marquand are mills into which the premises are fed and which turn out the conclusions by the revolution of a crank. The numerous mathematical engines that have been found practically useful, from Webb's adder up to Babbage's analytical engine (which was designed though never constructed), are also machines that perform reasoning of no simple kind. Precisely how much of the business of thinking a machine could possibly be made to perform, and what part of it must be left for the living mind, is a question not without conceivable practical importance; the study of it can at any rate not fail to throw needed light on the nature of the reasoning process. Though the instruments of Jevons and of Marquand were designed chiefly to illustrate more elementary points, their utility lies mainly, as it seems to me, in the evidence they afford concerning this problem.

The machine of Jevons receives the premises in the form of logical equations, or identities. Only a limited number of different letters can enter into these equations—indeed, any attempt to extend the machine beyond four letters would complicate it intolerably. The machine has a keyboard, with two keys for the affirmative and the negative form of each letter to be used for the first side of the equation, and two others for the second side of the equation, making four times as many keys as letters. There is also a key for the sign of logical addition or aggregation for each side of the equation, a key for the sign of equality, and two full stop keys, the function of which need not here be explained.[1] The keys are touched successively, in the order in which the letters and signs occur in the equation. It is a curious anomaly, by the way, that an equation such as A=B, which in the system of the transitive copula would appear as two propositions, as All A is B and All B is A, must not be entered as a single equation. But although the premises outwardly appear to be put into the machine in equations, the conclusion presents no such appearance, but is given in the form adopted by Mr. Mitchell in his remarkable paper on the algebra of logic. That is to say, the conclusion appears as a description of the universe of possible objects. In fact, all that is exhibited at the end is a list of all the possible products of the four letters. For example, if we enter the two premises All D is C, or D=CD, and All C is B, or C=BC, we get the conclusion in the following shape, where letters in the same vertical column are supposed to be logically multiplied, while the different columns are added or aggregated:

A	A	A	A	a	a	a	a
B	B	B	b	B	B	B	b
C	C	c	c	C	C	c	c
D	d	d	d	D	d	d	d.

[1] Phil. Trans. for 1870.

The capital letters are affirmatives, the small letters negatives. It will be found that every column containing D contains B, so that we have the conclusion that All D is B, but to make this out by the study of the columns exhibited seems to be much more difficult than to draw the syllogistic conclusion without the aid of the machine.

Mr. Marquand's machine is a vastly more clear-headed contrivance than that of Jevons. The nature of the problem has been grasped in a more masterly manner, and the directest possible means are chosen for the solution of it. In the machines actually constructed only four letters have been used, though there would have been no inconvenience in embracing six. Instead of using the cumbrous equations of Jevons, Mr. Marquand uses Professor Mitchell's method throughout.[1] There are virtually no keys ex-

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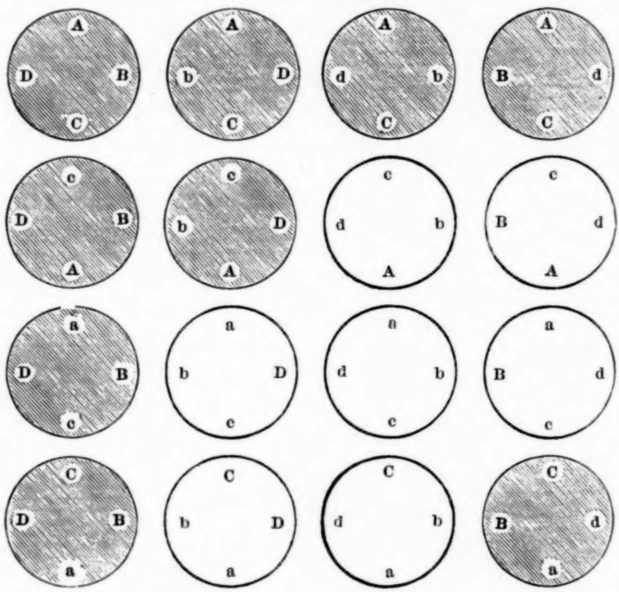
[1] It would be equally true to say that the machine is based upon Mrs. Franklin's system. The face of the machine always shows every possible combination ; putting down the keys and pulling the cord only alters the appearance of some of them. For example, the following figure represents, diagrammatically, the face of such a machine with certain combinations modified :



This face may be interpreted in several different ways. First, as showing in the shaded portions—

cept the eight for the letters and their negatives, for two keys used in the process of erasing, etc., should not count. Any number of keys may be put down together, in which case the corresponding letters are added, or they may be put down successively, in which case the corresponding combinations are multiplied. There is a sort of diagram face, showing the combinations or logical products as in Jevons's machine, but with the very important difference that the two dimensions of the plane are taken advantage of to arrange the combinations in such a way that the substance of the result is instantly seen. To work a simple syllogism, two pressures of the keys only are necessary, two keys being pressed each time. A cord has also to be pulled each time so as to actualize the statement which the pressure of the keys only formulates. This is good logic: philosophers are too apt to forget this cord to be pulled, this element of brute force in existence, and thus to regard the *solvet ambulando* as illogical. To work the syllogism with Mr. Jevons's machine requires ten successive movements, owing to the relatively clumsy manner in which the problem has been conceived.

One peculiarity of both these machines is that while they perform the inference from (A+B)C to AC+BC, they will not perform the converse inference from AC+BC to (A+BC). This is curious, because the inference they refuse to perform seems to be merely syllogistic, while the one they do perform, and in fact continually insist on performing, whether it is wanted or not, is dilemmatic, and therefore essentially more complicated. But in point of fact neither of the machines really gives the conclusion of a pair of

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$$\begin{array}{llll}(A+B+C+D) & (A+b+C+D) & (A+b+C+d) & (A+B+C+d) \\ (A+B+c+D) & (A+b+c+D) & & \\ (a+B+c+D) & & & \\ (a+B+C+D) & & & (a+B+C+d),\end{array}$$

which is the same as what is seen on the unshaded portions if we regard the small letters as affirmative and the capitals as negative, and interchange addition and multiplication, that is, as—

$$\begin{array}{rl} & aBCD+abCD \\ +ABCd+ & ABCD+AbCD \\ +ABcd+ & ABcD.\end{array}$$

Or, looking at the unshaded portion, we may regard it as the negative of the above, or—

$$\begin{array}{lll} & (A+b+c+d) & (A+B+c+d) \\ (a+b+c+D) & (a+b+c+d) & (a+B+c+d) \\ (a+b+C+D) & (a+b+C+d), & \end{array}$$

or, what is the same thing, as—

$$\begin{array}{ll}abcd+aBcd+aBcD+abcD & \\ +abCd+aBCd & \\ +AbCd & \\ +Abcd & +AbcD.\end{array}$$

There are two other obvious interpretations. We see, then, that the machine always shows two states of the universe, one the negative of the other, and each in two conjugate forms of development. In one interpretation simultaneously impressed terms are multiplied and successively impressed combinations added, and in the other interpretation the reverse is the case.

syllogistic premises; it merely presents a list of all the possible species in the universe, and leaves us to pick out the syllogistic conclusions for ourselves. Thus, with Marquand's machine, we enter the premise All A is B in the form a+B, and the premise All B is C in the form b+C; but instead of finding the conclusion in the form a+C, it appears as—

$$\begin{array}{l} ABCD+ABCd \\ +aBCD+aBCd+abCd+abCD \\ \qquad +abcd+abcD. \end{array}$$

As we only want a description of A, we multiply by that letter, and so reduce the conclusion to ABCD+ABCd, but there is no elimination of the B nor of the D. We do not even get the full conclusion in the form ab+BC, although it is one of the advantages of Marquand's machine that it does give the conclusion, not only in the form just cited, but also, simultaneously, as

$$\begin{array}{l} (a+B+c+d)\ (a+B+c+D) \\ (a+B+C+d)\ (a+B+C+D)\ (a+b+C+D)\ (a+b+C+d) \\ \qquad (A+b+C+D)\ (A+b+C+d). \end{array}$$

The secret of all reasoning machines is after all very simple. It is that whatever relation among the objects reasoned about is destined to be the hinge of a ratiocination, that same general relation must be capable of being introduced between certain parts of the machine. For example, if we want to make a machine which shall be capable of reasoning in the syllogism

If A then B,
If B then C,
Therefore, if A then C,

we have only to have a connection which can be introduced at will, such that when one event A occurs in the machine, another event B must also occur. This connection being introduced between A and B, and also between B and C, it is necessarily virtually introduced between A and C. This is the same principle which lies at the foundation of every logical algebra; only in the algebra, instead of depending directly on the laws of nature, we establish conventional rules for the relations used. When we perform a reasoning in our unaided minds we do substantially the same thing, that is to say, we construct an image in our fancy under certain general conditions, and observe the result. In this point of view, too, every machine is a reasoning machine, in so much as there are certain relations between its parts, which relations involve other relations that were not expressly intended. A piece of apparatus for performing a physical or chemical experiment is also a reasoning machine, with this difference, that it does not depend on the laws of the human mind, but on the objective reason embodied in the laws of nature. Accordingly, it is no figure of speech to say that the alembics and cucurbits of the chemist are instruments of thought, or logical machines.

Every reasoning machine, that is to say, every machine, has two inherent impotencies. In the first place, it is destitute of all originality, of all initiative. It cannot find its own problems; it cannot feed itself. It cannot direct itself between different possible procedures. For example, the simplest proposition of projective geometry, about the ten straight lines in a plane, is proved by

von Staudt from a few premises and by reasoning of extreme simplicity, but so complicated is the mode of compounding these premises and forms of inference, that there are no less than 70 or 80 steps in the demonstration. How could we make a machine which would automatically thread its way through such a labyrinth as that? And even if we did succeed in doing so, it would still remain true that the machine would be utterly devoid of original initiative, and would only do the special kind of thing it had been calculated to do. This, however, is no defect in a machine; we do not want it to do its own business, but ours. The difficulty with the balloon, for instance, is that it has too much initiative, that it is not mechanical enough. We no more want an original machine, than a house-builder would want an original journeyman, or an American board of college trustees would hire an original professor. If, however, we will not surrender to the machine, the whole business of initiative is still thrown upon the mind; and this is the principal labor.

In the second place, the capacity of a machine has absolute limitations; it has been contrived to do a certain thing, and it can do nothing else. For instance, the logical machines that have thus far been devised can deal with but a limited number of different letters. The unaided mind is also limited in this as in other respects; but the mind working with a pencil and plenty of paper has no such limitation. It presses on and on, and whatever limits can be assigned to its capacity to-day, may be over-stepped to-morrow. This is what makes algebra the best of all instruments of thought; nothing is too complicated for it. And this great power it owes, above all, to one kind of symbol, the importance of which is frequently entirely overlooked—I mean the parenthesis. We can, of course, dispense with parentheses as such. Instead of $(a+b)\ c=d$, we can write $a+b=t$ and $t\ c=d$. The letter t is here a transmogrified parenthesis. We see that the power of adding proposition to proposition is in some sort equivalent to the use of a parenthesis.

Mr. Marquand's machines, even with only four letters, facilitate the treatment of problems in more letters, while still leaving considerable for the mind to do unaided. It is very desirable a machine on the same principle should be constructed with six letters. It would be a little more elegant, perhaps, instead of two keys to each letter, to have a handle which should stand up when the letter was not used, and be turned to the right or left, according as the letter was to be used, positively or negatively. An obvious extension of the principle of the machine would also render it possible to perform elimination. Thus, if six letters, A, B, C, D, E, F, were used, there could be an additional face which should simply take no notice of F, a third which should take no notice of F or E, a fourth which should take no notice of F, E or D; and these would suffice. With such a machine to represent AB+CD, we should proceed as follows: Put down handle E to the left. [The left hand would naturally signify the negative.] Leaving it down, put down handle A to the right and then bring it back after pulling the cord. Put down handle B to the right and pull the cord, and then restore handles B and E to the vertical. Next, put down handle F to the left and successively put down the handles C and D to the right, as before. After restoring these to the vertical, put down handles E and F to the right, and pull the cord. Then we should see on the third face

(A+B+C+D) (A+b+C+D) (A+b+C+d) (A+B+C+d)
(A+B+c+D) (A+b+c+D)
(a+B+c+D)
(a+B+C+D) (a+B+C+d)

or, what comes to the same thing,

aBCD+abCD
ABCd+ABCD+AbCD
ABcd+ABcD

I do not think there would be any great difficulty in constructing a machine which should work the logic of relations with a large number of terms. But owing to the great variety of ways in which the same premises can be combined to produce different conclusions in that branch of logic, the machine, in its first state of development, would be no more mechanical than a hand-loom for weaving in many colors with many shuttles. The study of how to pass from such a machine as that to one corresponding to a Jacquard loom, would be likely to do very much for the improvement of logic.

C. S. PEIRCE.

The Functions of the Brain. By DAVID FERRIER, M. D., LL.D., F. R. S. Second Edition. G. P. Putnam's Sons. New York, 1886. Pp. xxiii., 498.

Die Functions-Localization auf der Groshirnrinde. Von L. Luciani und G. Seppilli. Deutsche Ausgabe von Dr. M. O. Fraenkel. Leipzig, 1886. Pp. vii., 414.

When the first edition of Ferrier's work appeared in 1876 it attracted the attention of English readers to the subject of the localization of brain functions, and made an important addition to the mass of facts which had already begun to accumulate upon that subject. Fritzsch and Hitzig had determined in 1870 the existence of a definite area on the surface of the brains of vertebrates, irritation of which produced movements of the limbs, and destruction of which caused paralysis. Ferrier not only confirmed the results of the German physiologists, but went a step farther and succeeded in demonstrating the existence of various sensory areas in the brain, destruction of which produced a loss of some one of the powers of conscious perception. It was very natural that results of such physiological importance should be tested carefully, and there is probably no field of inquiry in which during the past ten years more active work has been done and more acrimonious controversy has arisen. Hardly any two investigators can be found who agree as to the extent of the various sensory areas, and the most different opinions as to the interpretation of the results of experiment have been defended. Ferrier's second edition is issued partly in order to offer new facts from new experiments and to modify former opinions in light of these new facts, partly in order to reply to criticism, and partly in order to defend his own interpretation of his facts. The work is almost wholly rewritten, and differs in so many respects from the first edition that it requires notice.

The work of Luciani and Seppilli received the Fossati prize of the Institute of Science of Lombardy, and was considered worthy of immediate translation into German, not only because of the new discoveries and new views contained in it, but also because of the

singularly unprejudiced and impartial views presented of the entire controversy. And this latter characteristic deserves to be commended, for much that is valuable in the writings of Ferrier, Goltz, Munk, and others, who have taken an active part in the discussion regarding the localization of functions, is obscured by the intensity of personal criticism and recrimination with which it is loaded down. The Italian authors, though holding very definite positions of their own, have succeeded in stating the views of opponents with fairness, and have suggested many probable interpretations of seemingly contradictory facts which may reconcile the inconsistent statements. They have added to the value of their work by appending to it a collection of cases of brain disease in man, which enables them to compare the results of physiological experiment with those of pathological observation.

In spite of the criticism of Goltz it must be admitted that the theory of localization has gained almost universal acceptance. Various areas of cortex of the brain are now admitted to preside over and to be necessary to various forms of mental activity. Certain parts of the cerebral cortex receive impulses from the sensory organs and preserve them as memories. Other parts send out voluntary impulses to the motor apparatus. The motor areas are definitely known and accepted. The controversy now among physiologists is regarding the exact limits of the different sensory areas. Ferrier lays down these areas in his diagrams as little circles, each separate from every other; and this extreme position must be admitted to be a logical consequence of the admission that localization is possible. Munk extends his areas somewhat more widely, does not limit them so exactly, and yet does not allow one area to invade the domain of the next. He goes even further than Ferrier in locating the visual sense, making different parts of the accepted cortex correspond to different parts of the retina. Luciani holds that each sensory area is extensive, and that, at its borders, it not only is not sharply marked off from, but really overlaps that of adjacent areas. Goltz admits that there is a functional difference between the anterior half and the posterior half of the brain, but will not allow that any distinct sensory or motor area can be outlined, claiming that the assertion of Flourens was correct and that the brain acts as a whole.

A distinction has been proposed by Exner which should be mentioned here. Exner believes that it is necessary to admit the existence of both absolute and relative functional areas. An absolute area is one, injury to which is always followed by loss of the function. A relative area is one, injury to which is sometimes but not always followed by a loss of the function. For each sense there is an absolute area, which is surrounded by an extensive relative area, and the relative areas for different senses may to some extent coincide. This distinction is virtually admitted by Luciani, whose experiments prove that loss of function is permanent when the absolute areas are destroyed, but may be temporary when the relative areas only are affected. It is not admitted by Ferrier, yet his own account of his experiments may be cited in favor of such a view, for he admits that in some of his experiments the function returned after the supposed centre was destroyed. (See p. 22 *et seq.*, where it is stated that destruction of the angular gyrus produces only transient loss of vision in the opposite eye, while if the destruction also involves the occipital lobe there is also permanent hemiopia).

It is not admitted by Munk, and yet his distinction in the case of disturbances of sight, between psychical and cortical blindness, might easily be referred to such a simple explanation. To an independent inquirer there seems to be much in favor of the position of Luciana and Exner, as it appears to reconcile in some degree the conflicting statements.

Any one who reads these statements carefully, and who also reads the detailed account of the experiments upon which they are based, must be impressed at once with the fact that the differences are due rather more to the interpretation of the experiments than to their actual results. It is a very difficult matter to ascertain just what functions are wanting, or to what degree any one function is impaired in an animal after an operation on the brain. The result which the observer looks for is the one most likely to be found, and as an animal cannot communicate its own sensations in language, much is left to be guessed at. It is for this reason that experimentation in animals seems to be of much less importance in deciding upon the location of sensory areas than the results of pathological observation in man. Munk, Exner, and Luciani appeal to pathology frequently and claim that it supports their various positions. Ferrier seems less inclined to admit this kind of evidence (p. 270), possibly because it fails to support some of the positions which he holds as opposed to other observers—*e. g.*, as to the location of the tactile centres and the visual centre. Yet it might be supposed that the English physiologist would insist upon this class of evidence, for he has directed a number of operations upon the human brain in cases of paralysis recently performed in London by Victor Horseley. Such cerebral operations are the practical outcome of the doctrine of localization, and have been its most brilliant confirmation. For the situation of the motor areas of the brain being agreed upon by all experimenters, it has been possible in cases of paralysis from brain tumor or abscess to trephine the skull and remove the disease from the brain, with the result of saving the life, and in some cases of restoring partially or wholly the function of the paralyzed limb. There is therefore a practical importance in determining the location of the sensory areas of the brain, in order that such operations may be performed when some one sense is lost as well as when some one limb is powerless. The importance of observations as to the effect of disease on the human brain, and the necessity of accurate localization of such disease after death, has not been exaggerated, and considerable effort is being made on all sides to collect and compare cases of such a kind. A definite settlement of the controversy does not seem to be far distant, for in respect to the function of sight authorities are now well agreed, the visual area of the human brain being undoubtedly in the occipital region.[1] We may hope for equally positive results regarding other areas.

The work of physiologists upon the cortex has brought to light the importance of distinguishing reflex and automatic activity from conscious volitional motion; and the subcortical perception of sensations, which always results in an automatic response, from the cortical perception of sensations, which is always conscious, and which may or may not give rise to action. And among the most interesting results is the determination of the facts that conscious mental-action perception, together with memory and volition, are

[1] E. C. Seguin, Jour. of Nerv. and Ment. Dis., Jan., 1886.

functions of the cortex alone; also, that the association of ideas is secured by the intimate union of various areas of the cortex, through the medium of nerve fibres passing just beneath the surface.

In view of the remarkable work which is being done at present in the department of psycho-physics in measuring the time of such processes of association, the study of the physical basis of the physiological process gains in interest.

The physiologists have succeeded in demonstrating the complex organic basis of memory by these experiments upon the cortex. It is now evident that we must speak rather of memories than of memory—each sensory or motor act leaving behind it a molecular change in the cortex which is to be regarded as the physical substratum necessary to recollection or reproduction. And as the memory of any single object is made up of a number of revived impressions, each derived through a separate sense, and each received in a different area, the mental image of the object involves the activity of various parts of the cortex, the revival of numerous, distinct memory-pictures, joined in a complex unit. It follows that a single set of memories may be lost by disease in one part of the brain, while other memories remain, a conclusion which is amply illustrated in the phenomena of aphasia. (Ferrier, pp. 440–460).

That there is any necessity for postulating "ideational centres" distinct from the correlated sensory and motor centres, is combatted by Ferrier. "We have in the sensory and motor centres of the cortex the substrata of the respective forms of sensory perception and ideation, and of the individual acts of volition, simple and compound, as well as of the feelings associated with their activity. It seems more reasonable to suppose that there may be higher and lower degrees of complexity or evolution in the same centres than to assume the separate existence of more highly evolved centres for which no evidence is obtained by the results of experimental research." (Page 460.) "Intelligence and will have no local habitation distinct from the sensory and motor substrata of the cortex generally. There are centres for special forms of sensation and ideation, and centres for special motor activities and requisitions, in response to and as association with the activity of sensory centres; and these, in their respective cohesions, actions and interactions, form the substrata of mental operations in all their aspects and all their range." (Page 467.)

The discussion of the psychological side of brain action is more intelligent and philosophical in the English than in the Italian work. But both of these books may be recommended for careful perusal to anyone who desires to become familiar with the facts upon which the theory of the localization of brain functions is based.

M. Allen Starr.

Francis Galton on the Persistency of Type.

In his opening address as President of the Anthropological section of the British Association, at its Aberdeen meeting, Francis Galton gave an account of his researchesregarding the inheritance of size in seed and of stature in man, as well as certain generalizations which he deduces from his observations. His observations

have been warmly and justly welcomed by all students of inheritance, as valuable contributions to our positive knowledge of a subject where the attainment of positive knowledge is peculiarly difficult; but it seems to me that, although the general conclusions are worded with the greatest care, they are in some respects misleading, and opposed to our general knowledge of the subject.

A few extracts will serve to exhibit the results of his observations and the character of his general deductions. He says (Nature, Sept. 4, 1885): "It is some years since I made an extensive series of experiments in the produce of seeds of different sizes, but of the same species. * * * It appears from these experiments that the offspring did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they; to be smaller than they if the parents were large; to be larger than the parents if the parents were very small," and that the analysis of the family records of the heights of 205 human parents and 930 children fully confirms and goes far beyond the conclusions he obtained from seeds, as it gives with great precision and unexpected coherence the numerical value of the regression towards mediocrity. He points out that this regression is a necessary result of the fact that "the child inherits partly from his parents, partly from his ancestors. Speaking generally, the further his genealogy goes back, the more numerous and varied will his ancestors become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre." He illustrates this by comparing the result of the combination in the child of the mean stature of the race with the peculiarities of its parents, to the result of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a certain fraction of its original alcoholic strength, whatever that strength may have been.

He then goes on to conclude that the law of regression to the type of the race "tells heavily against the full hereditary transmission of any rare and valuable gift, as only a few of many children would resemble their parents. The more exceptional the gift, the more exceptional will be the good fortune of a parent who has a son who equals, and still more if he has a son who surpasses him." The law is even-handed; it levies the same heavy succession tax on the transmission of badness as well as goodness. If it discourages the extravagant expectations of gifted parents that their children will inherit all their powers, it no less discountenances extravagant fears that they will inherit all their weaknesses and diseases." * * * "Let it not be supposed for a moment that" the "figures invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair; what it asserts is that the ablest children of one gifted pair is not likely to be as gifted as the ablest of all the children of many mediocre pairs."

My first criticism of Galton's data as a basis for generalization is that they are misleading in so far as they fail to discriminate between the persistency of hereditary and that of non-hereditary parental peculiarities.

The departure of a human being from the normal stature of the race may be an example of any one of three classes of phenomena:

1. It may be physiological, or due to influences which during the life of the individual tend to dwarf or to develop it. The short

stature of sailors is usually attributed to this cause, and it is possible that the size of the seeds and the stature of some of the human parents was physiological. In this case there is no reason to expect any tendency towards perpetuation.

2. A variation may be congenital but not hereditary, as when a single giant or dwarf is born in a family where the ancestors and descendants have the normal stature.

3. The peculiarity may be congenital and hereditary, as it is when a certain stature is characteristic of the brothers, sisters, and colateral relatives of a parent; when it is a family characteristic, or when it is characteristic of a variety of the human race, like the Bushmen.

There is ample evidence that the persistency, in the descendants, of a parental peculiarity varies greatly according as it belongs to one or the other of these classes, and we know that, quite independently of any selection, a hereditary peculiarity—that is, one which is shared by all the members of a family—often shows an astonishing tendency to persist in later generations, quite independently of the time during which it has already persisted.

A most remarkable illustration may be found on page 30 of Professor Bell's memoir on "The Formation of a Deaf Variety of the Human Race." (Mem. Nat. Acad. of Sc., Nov., 1883).

In the H. family, of Kentucky, two brothers and a sister inherited from their parents a common predisposition towards deafness, as is shown by the fact that they all became the ancestors of congenital deaf mutes, although only one of them was deaf. We have no information regarding the first generation, the parents, but in the second generation one of the three children was deaf. In the third generation all the descendants, eleven in number, were deaf. In the fourth generation the record is incomplete, but all the children which are known, six in number, were deaf. In the fifth generation selection was introduced, as three of the children married deaf mutes. The records are very incomplete, but of the six descendants known one was deaf.

The genealogy of this family is given in the following table, which serves to show that, in case of a hereditary peculiarity, the tendency of the children to resemble their parents may be vastly greater than their tendency to revert to the normal type of the race.

First generation.	No information concerning their hearing.				
Second generation.	Son deaf.	Daughter hearing.	Daughter hearing.		
Third generation.	Seven deaf children.	Two deaf children.	Two deaf sons.		
Fourth generation.	No information concerning the descendants.	One child had two deaf children; no information concerning the other.	One son did not marry; the other had two deaf daughters, $D,^1$ $D,^2$ and one deaf son, S.		
			D^1 married a deaf man.	D^2 married a deaf man.	S married a deaf woman.
Fifth generation.	No information.	No information.	One deaf son.	No children.	Five hearing children.

I find among some notes which Professor Bell has kindly placed in my hands another interesting case. O. H. was the only deaf child in a family of eleven children. He had four children, two of them deaf, and three grandchildren, two of them deaf, so that the relative predisposition of his parents, himself and his children to transmit deafness may be represented by three fractions, $\frac{1}{11}$, $\frac{1}{2}$, $\frac{2}{3}$.

It is only in a figurative sense that we can say that a child is the offspring of remote ancestors as distinguished from its parents; for even if we believe in the continuity of germinal protoplasm, it still remains true that all the matter in the fertilized egg comes from the parents, and the history of the Kentucky family shows that a hereditary variation, even when it is not very ancient, may be much more potent than all the influence which comes from ancestry.

These facts, and many more which might be quoted from our stock of information regarding domesticated animals and plants, show that if Galton had studied the persistency of *hereditary* peculiarities of stature, independently of selection, his results might have been quite different, and the experience of all breeders shows that if he had tabulated by themselves the cases where the parents had the *same* hereditary peculiarity of stature, where selection had been exercised, his general conclusion would be quite inapplicable to the result.

A few months after Galton's paper was printed another paper appeared by a well-known authority. (Die Bedeutung der sexuellen Fortpflanzung für die Selections-Theorie, Weismann, Jan., 1886), and on page 40 I find the statement that "when the same part is greatly developed in both parents, the experience of breeders shows that it is still more developed in the children."

It is undoubtedly true that the average child is less aberrant than the parents, and that each child inherits a tendency to revert, or, as Galton shows, to lie midway between its parents and the type of the race; but it is also true that when both parents have the same peculiarity there is a very considerable probability that some children will equal or surpass them, so that the peculiarity may be rapidly culminated by selection.

Galton overlooks the fact that the "type" itself is not a fixed quantity, since it admits of rapid modification by the continual selection of such slight variations as constantly present themselves under the ordinary and normal conditions of life.

It seems to me that the following observations disprove his statement that "the appearance of a new type is due to causes beyond our reach," as they show that the type, that is, "*the ideal form towards which the children of those who deviate from it tend to regress*" (*Galton*) may itself be rapidly modified by selection.

The observations are given by Fritz Müller in a recent number of Kosmos (Ein Zuchtungsgesuch an Mais, Kosmos, 1886, 2, 1, p. 22).

Yellow corn is very variable in many respects. The number of rows of kernels on the cob is usually from 8 to 16, cobs with 10 or 12 rows being the most common, while one with 18 or 20 rows is very seldom found. A search through several hundred cobs gave him one with 18 rows, but none with more.

In 1867 he sowed at different times, and in such a way as to prevent crossing, (1) the seed from the cob with 18 rows, (2) the seed from the finest 16 rowed ears, and (3) the seed from the finest 14 rowed ears. In 1868 he sowed (1) seed from a 16 rowed ear

which had grown from seed from a 16 rowed ear, (2) seed from an 18 rowed ear from 16 rowed seed, and (3) seed from an 18 rowed ear from 18 rowed seed.

In 1869 he sowed (1) seed from an 18 rowed ear with 18 rowed parents and grandparents, (2) seed from a 20 rowed ear with 18 rowed parents and grandparents, and (3) seed from a 22 rowed ear from seed from an 18 rowed ear, produced from seed from a 16 rowed ear. The results are given in the accompanying table:

	1867.			1868.			1869.		
Number of rows on cob from which *seed* was taken....	14	16	18	16 16	16 18	18 18	18 18 18	18 18 20	16 18 22
No. of cobs produced..	658	385	205	1789	262	460	2486	740	373
	%	%	%	%	%	%	%	%	%
8-rowed cobs..........	.3	......	.5	.1	......	......	......	......	......
10 "	14.4	3.	1.	1.4	.8	.2	.1	......	......
12 "	48.0	22.8	13.	22.6	14.5	7.8	6.1	6.1	2.7
14 "	35.6	48.6	37.8	48.5	46.7	35 4	37.3	28.5	25.3
16 "	3.2	18.7	34.5	22.2	23.7	33.8	33.5	41.6	41.8
18 "	.5	6.8	12.6	4.9	12.3	18.2	18.6	20.2	24.1
20 "	......	.1	.3	.3	1.2	4.4	3.9	2.8	4.8
22 "	......	......	.3	......	.8	.2	.5	.8	1.
26 "	......	......	......	......	......	......	......	......	.5
Average	12.61	14.08	14.9	14.15	14.39	15.52	15.57	15.76	16.15

It will be seen from this table that the number of ears with few rows decreases very rapidly in children produced from seed taken from ears with many rows, and that the greater the number of rows on the ear from which seed is taken, the smaller is the number of ears produced with a small number of rows. It is also plain that as the number of rows on the ear from which seed was taken increases, the number of ears produced with a large number of rows increases, and that we have in each case a very considerable number of ears which equal their parents and a few which excel them, even when the parent seeds are far beyond the maximum of all ordinary corn.

Fritz Müller says that he has never, except in three instances, found an ear with more than 18 rows, and Darwin in his "Variation" puts the maximum at 20 rows, yet we have in the children of seed from a 22 rowed ear no less than 4.8 per cent., or no less than 18 ears out of 373 with 20 rows, and one ear out of 373 with 26 rows. I am quite unable to reconcile this result with Galton's statement "that the ablest children of one gifted pair are not likely to be as gifted as the ablest of all the children of many mediocre pairs." It is undoubtedly true that if Müller had planted in 1869 all the seed from the 2,511 ears which he raised in 1868, instead of planting seed from only three ears, the chance of finding among the descendants ears with 26 or more rows would have been somewhat increased. In this case, however, the parents would not have been mediocre, for nearly all of them were above, and many of them far above, the average for the race, and the chance of finding in ordinary corn an ear with 26 rows is so small that it may be treated as zero.

The results also seem strongly opposed to Galtin's statement that his law tells heavily against the full hereditary transmission of any

rare and valuable gifts, for an examination of the table will show that the number of children which resemble their parents increases in this case with each successive generation. Thus the seed planted in 1867 from an ear with 18 rows produced 12.6 per cent. of 18 rowed children. The 18 rowed ear planted in 1868 from an 18 rowed parent cob produced 18.2 per cent. of 18 rowed children, and the 18 rowed seed planted in 1869 from 18 rowed parents and grandparents produced 18.6 per cent of 18 rowed children. The series is 12.6 per cent., 18.2 per cent., 18.6 per cent.

A percentage of 18 gifted children to the hundred may be discouraging to the "extravagant expectations of gifted parents that their children will inherit all their powers," but it is a most potent factor in the process of race modification by selection.

Müller's table shows, like Galton's observations, that the greatest number of children are not like the parents, but intermediate between them and the "type" or the average for the race. This is exhibited in the following table, in which the number of ears in the parent cob is given in the left-hand column, and the percentage of ears with the same number of rows, produced by the children in the second column, and the percentage of ears produced with the dominant number of rows in the third column.

1867	14	14 rows 35.6 %	12 rows 48 %
1867	16	16 " 18.7 %	14 " 48.6 %
1868	16	16 " 22.2 %	14 " 48.5 %
1867	18	18 " 12.6 %	14 " 37.8 %
1868	18	18 " 18.2 %	14 " 35 4 %
1869	18	18 " 18.6 %	14 " 37.3 %
1869	20	20 " 2.8 %	16 " 41.6 %
1869	22	22 " 1. %	16 " 41.8 %

It is thus seen that, like stature, the number of rows tends to revert to the type, but then it will also be seen that, in only three generations, the type itself may be so greatly modified by selection, that the minimum of the third generation may be equal to the mean of the first generation, and the mean of the third generation, 16 rows, is in this case very near the maximum for accidental ears.

W. K. Brooks.

E'tudes experimentales sur les illusions statiques et dynamiques de direction pour serir a determiner les fonctions des canaux demi-circulaires. Par Yves Delage. Archives d' Zool. Exper. No. 4, 1886. pp. 535–624, (with index.)

Since the days of Flourens there have appeared few more valuable contributions to the physiology of the sense of equilibrium and of the semi-circular canals than this work of Professor Delage. The author goes far toward reconciling the conflicting opinions of those who, on the one hand, hold that the semi-circular canals are special spatial sense-organs, on whose activity depends every sense of position or direction of movement of the body; and of those who, on the other hand, think there is no good evidence of a normal relation between these organs and the sense of equilibrium. The general question is this: When the eyes are closed, through what sense or senses do we derive ideas of the direction of objects in

space, and of the position of our bodies with reference to them both while we are at rest and in motion? Since any sense-organ, when placed under abnormal conditions, may give rise to illusions dependent on such organ alone, a study of sensory illusions is a valuable aid in determining through what physiological channel any particular information is usually received. When visual sensations are excluded we still have distinct ideas of the direction of the various objects about us while our bodies are at rest, and of motion and direction of motion when our bodies are moved. The subject may, therefore, be conveniently divided into a study of *static* and *dynamic* sensations and illusions.

When the body is at rest and the eyes are closed what is the sense which gives us a knowledge of the direction of things in space? An observer standing upright, with eyelids closed and his visual axis directed straight forwards, can indicate without error the direction of any object in space and the position of the various parts of his own body. But if the head be turned as far as possible about one of its axes, the judgments of direction become false, and the observer points out directions as if external space had revolved through an angle of some fifteen degrees about the head in its normal position, and in the same plane, but in a direction opposite to that of the true motion. This indicates that the organ for which we depend for static sense of direction lies in the head. Is this organ the internal ear? If the observer turns his eyes alone, while the head remains at rest, illusion is the same as before; if the head is turned, while the eyes are forced to retain their original resting position, the illusion disappears. It can be shown that the illusion has its origin in the fact that when both head and eyes are turned the eyes unconsciously move through a greater angle than the head, which is equivalent to a positive rotation of the eyes within the unmoved head, and as this contraction of the eye-muscles gives rise to an unconscious sensation, it appears as if external space had rotated through an equal angle and in an opposite direction. It is concluded, therefore, that static sensations of direction comes as through the eye-muscle and not through the semi-circular canals.

Through what means do we gain a knowledge of the position of our bodies? If an observer, with eyes closed and the head in a normal position, is supported with his back upon a board which can be revolved about a horizontal axis, as the head end of the board is inclined toward the horizon, the observer rightly estimates his position when the inclination is about 60°; at angles of less value he judges his inclination to be slightly less than the reality, but after passing that angle the error rapidly increases in the other direction, so that when the board is inclined at an angle of 120°, the body seems to be vertical, with head downward. These results contradict those of Mach, whose experiments were performed in essentially the same manner. Sensations derived from the eye-muscles would tend to correct rather than increase this illusion of position, and sensations from the internal ear have no share in it, for the illusion can only be modified by altering the muscular and cutaneous sensations involved in the change of position.

It is concluded that our knowledge of the position of the body under these conditions depends upon muscular and cutaneous sensations, together with that general sensibility which appreciates the direction of gravitation of the fluids and internal organs of the body.

Dynamic sensations may be divided into those produced by rotation of the body and by simple translation in a straight line. We are very sensitive to movements of rotation imparted to our bodies. Our author, like previous observers, has found that when the motions are of short duration we can judge not only angular accelerations, but angular velocities, and the value of the angles traversed. It is only after prolonged rotation, a condition not experienced in ordinary life, that uniform motion is attended by failure of sensation with feeling of motion in the opposite direction after arrest. But even during continuous rotation, according to Delage, we are conscious of variations of velocity, and not, as held by Mach, of changes of acceleration alone. The movements of rotation were performed, with visual sensation excluded, round each of the three principal axis of the body. In turning round the vertical axis the observer was inclosed in a box to which air but no light was admitted, and which was suspended by two ropes, the twisting and untwisting of which gave any desired velocity of rotation. In movements round the other axes the observer was supported upon revolving table, which could be inclined at any desired angle. If, during a movement of rotation, the position of the head be changed with reference to the body, the axis of rotation itself appears to be changed in the same plane as the head, and with an equal angle, but in the opposite direction. We attribute to our body such a motion as it would have were it prolonged in its natural relation from the head in its new position. It seems clear from these facts that the organ which gives us sensations of rotation resides in the head. This organ cannot be the eye, for it was found that the ocular sensations produced by rotation are less powerful than those really experienced, and of the opposite sign. The semi-circular canals of the internal ear, from their anatomical structure and by the motor results following injury of them, seem to be the organs on which these sensations of rotation depend, and the current explanation of their operation through the excitement of their auditory nerve filaments, due to variation of endolymph pressure as the head is turned round its different axes is probably the correct one.

The sensations produced, with eyes closed, by translation of the body in a straight line, are much less delicate than those aroused by rotation. When the movement is of short duration, a fairly correct judgment is formed of its velocity, amplitude and duration. When long continued, the sense of motion fails. Sudden arrest of the motion does not give a sensation of translation in the opposite direction There is no illusion as to the direction of motion when the position of the head alone is changed; therefore, the origin of the sensation is not in this part. The sensations of translation seem to have a general source, and depend upon the varied pressure of the internal organs and of the fluids throughout the body.

In brief, it may be said that M. Delage admits that the sense of equilibrium is supplied by sensations having several different sources. Besides the purely visual sensations, we depend for our knowledge of direction, either static or dynamic, upon feelings derived from the ocular muscles; upon sensations of touch, and upon general muscular sensibility; upon a feeling of the direction of pressure of the general fluids and of the internal organs of the body, as well as upon a special function of the semi-circular canals of the internal ear. The semi-circular canals are stimulated chiefly,

or only, by rotatory movements of the head, and seem to be special sense-organs for this kind of motion alone. Our appreciation of such motions is extremely delicate, as, indeed, should be expected when it is considered that it is upon movement of the head about one of its axes that we depend in every-day life for our judgments concerning our motions, and our change of position with reference to surrounding objects.

HENRY SEWALL.

Das Körperliche Gefühl. Ein Beitrag zur Entwickelungsgeschichte des Geistes. von Dr. EUGEN KRÖNER. Breslau, 1887. pp. 207.

The point of view from which this work is written is that of the naturalist and the evolutionist. As an outcome of the modern biological renaissance there has resulted the science of physiological psychology. To ensure the progress of this movement up to the stage of the exact sciences, two methods must be employed, the experimental (psycho-physics) and the comparative (genetic.) The latter is the method by which feeling is to be studied. The chief problems are—(1.) What psychic activities has the new-born infant? (2.) How are the faculties of the adult evolved from these? It is soon found that these problems are insoluble without the consideration of the development of psychic functions along the animal scale. As in bodily so in mental evolution, the two progress in parallel lines. Häckel's biogenetic law that "autogenesis, or the development of the individual, is a rapid and condensed repetition of the phylogenesis, or the development of the species," must be applied to psychology. Hence the importance of animal psychology and especially does this hold of the study of the feelings.

The lack of this genetic method of regarding emotional phenomena is the common fault of all historical systems, and one of the greatest obstacles in the way of such a conception was the conventional trinity of faculties with reason as the chief and fundamental. From the genetic point of view, feeling is the primary fact of life. It is the fundamental property common to all irritable tissue. The differentiation of subject and object, on which all reason depends, requires a more or less specialized sense organ, and such does not exist in the lower forms of life. The lowest stage in this evolution is represented by the conæsthesic feelings (Gemeingefühl). These are caused by the getting into consciousness of physiological activities, and are characterized by their vagueness—lack of localization—and by being pleasure-giving or the reverse. The first days of infant life are spent in this sphere. (Romanes puts the psychic life of a new-born child on a level with that of the coelenterata.) The next higher stage appears in sense-feeling (*sinnliche Gefühl, betonte Empfinding* of Herbart), in which the pleasurable or powerful effect is the concomitant of a more or less definite sensation. The distinction between the two is considered of radical importance.

The filling out of this plan is done with as great accuracy as our present knowledge will allow, while the treatment is everywhere interspersed with useful illustrative details. Theory is not resorted to when facts are the criterion, nor is introspection—nowhere so dangerous an instrument as here—allowed to rule over objective verifiable truth. Dr. Kröner's book may be recommended as the

most useful compilation of this chapter of physiological psychology that has yet appeared. It owes much to the marked analysis of Horwicz, but differs from that author in several respects. A too frequent mention of Dr. Jäger is perhaps the only point of fault-finding which may safely be indulged in, without bordering on hypercriticism, which in this difficult field is especially out of place.

J. JASTROW.

Die Seelenblindheit als Herderscheinang und ihre Beziehungen zur Homonymen Hemianopsie zur Alexie und Agraphie. Von DR. HERMANN WILBRAND. Wiesbaden, 1887. pp. 192. 8vo.

No question in the study of localization of brain functions has called forth such a voluminous and violent controversy as that of the centres of vision. No other question has led to such important and suggestive conceptions of the nature of brain centres, or has been attacked by so many and such ingenious methods. When Munk destroyed certain regions of the dog's brain and found as the permanent result a loss of the memory-pictures of sight, while the animal used its eyes to avoid obstacles, etc., as before, he gave to this condition the name of "psychic blindness" (Seelenblindheit). The dog could see as long as his lower optical centres were intact; to recognize and interpret what he saw required the higher cortical centres.

A precisely analagous condition is produced by cortical disease in man. Dr. Willbrand gives two classical cases of this nature, one from Charcot's clinic, the other from his own. In both these cases the intelligence was intact and the description of the symptoms by the patient extremely definite and valuable. Charcot's case is especially conclusive, because the subject of it possessed before his trouble a remarkable visualizing faculty. He could read pages of his favorite authors from the mental picture of the printed book which appeared before him; when he thought of a certain spot he visualized a complete colored photograph of it. During his attack all this had to be transferred to the ear, and to remember anything he had to repeat it *aloud* to himself.

The conclusions to which Dr. Willbrand's study leads him are briefly these: If the conduction of impressions along the optic tract be hindered, blindness in the ordinary sense is the result. But visual hallucinations, dream-visions and subjective light sensations are possible, and the memory of the world of sight remains. If the perceptive centre of one hemisphere is destroyed, unilateral cortical blindness ensues, appearing as an absolute and complete hemianopsia of the opposite holds of the field of visions. If both hemispheres are thus affected, hallucinations and subjective vision are impossible, but the memory of seen objects need not be impaired.

If, however, it is the "optical memory areas" that are affected, form and color may be seen, but they make an unfamiliar impression. The visual phantasy gradually atrophies, and dreams become visionless. Subjective light-impressions remain. It must also be remarked that these phenomena are liable to complication by loss of names for visual objects.

Dr. Willbrand's book will take its place amongst the most valuable contributions to this intricate subject, which perhaps more than any other offers a promising path to a deeper knowledge of

the nature of psychic functions. The value of the book is enhanced by a chapter describing the process of learning to see in those who, born blind, have been restored to sight.

J. JASTROW.

Physiologische Studien über Psychophysik. Von DR. FRANZ CARL MÜLLER. Archiv. für Anat. u. Physiol., Heft III. u IV. 1886.

This third German investigator of his name in the field opened by Weber's law, attempts to determine how the negative variation needful to excite a just observable contraction is related to absolute intensity of the [ascending] current. For this purpose a single pair of unpolarized electrodes served for the permanent and for the reversing current in such a way that when the contact was closed the currents were separated and compensated; when it was open they combined, causing a negative variation. The sciatic nerve of a frog was first observed, and the minimal contractions of the toe-muscles directly observed. The quotient of the intensity of the larger current (measured in divisions of the current passed over by the needle of the galnometer), divided by that of the varible current, here measures the psychophysic relation sought. This quotient begins in feeble currents with a threshold intensity of unity, and increases rapidly to two or three times its initial value, and then remains constant for a time, till with very strong currents it sinks again. Although the first period of increasing differential sensibility is quite analgous to the lower limit of Weber's law, the second period of constant quotients, the extent of which differs for different nerves, is especially important. Very similar results were attained on rabbits and guinea pigs. Percutaneous stimulation by the same method on the motor points of various digital muscles in the human arm, gave results with somewhat greater irregularity, but with a long second period of constant quotients. Next, instead of just observable muscular contraction, just observable differences of sensation were attempted with similar results. From these experiments Dr. Müller feels himself justified in calling Weber's law only one [psycho-physic] case of a larger "neurophysic" law which applies to all stimuli that diminish excitability, and formulates his law as follows: "The excitation caused by a change of intensity of a stimulus that diminishes excitability remains the same (under conditions otherwise similar and within certain limits of absolute intensity of stimulus), if the relation of the change of intensity to the intensity on the basis of which the change is made remains the same. Outside these limits, with constant relation between intensity and change of intensity from one degree of stimulus to the next higher, an increase of excitation occurs with small, and a decrease with great intensity."

A sensation of difference which Fechner substituted for Weber's difference of sensation, is not a sensation at all, but a judgment. A sensation due to a constant stimulus is physiologically a state of diminished excitability. Changed excitability is thus an essential property of sensation which serves as an index to the inner dynamic, or neurotonic state of the nerve. The act of bringing two sensations, or even the memory of two past sensations into relation, or comparing them by alternating from one to another, is the simplest form of any judgment, and is physilogically represented by

excitation attendant on the transition from one state of excitability to another. As a state of reduced excitability, the psycho-physic process that underlies sensations is thus directly proportional to the intensity of the stimulus. By using as his stimulus the change from anelectrotonus to katelectrotonus, and correcting for the movement of the indifference point along the myopolar tract, Müller was able to study states of increased excitability, and the effects of transition from reduced to increased excitability, although in a preliminary way, respecting which fuller results are promised. His work is in the line of Dewar and McKendrick, Bernstein and Ward, indicating that the sensation is directly as nervous action, and that the logarithmic relation holds between the stimulus and the amount of neural excitation. He attempts, however, to subsume Fechner and Weber under the law of stimulation by changed electrotonic state. The author's fuller results will be awaited with interest.

Untersuchungen uber das Tongedächtness. Von H. K. Wolfe. Wundt's Philosoph. Studien. III. 4.

Dr. Ebbinghaus studied the function of memory as a reproductive faculty by counting the number of repetitions of a variable series of nonsense syllables, necessary to enable the learner to repeat them at will after a given interval, also subject to variations. Mr. Wolfe's study is upon memory as a recognizing faculty; evidently an easier and more extended power. In a second reading of a book, for example, we recognize as familiar much more than we could have repeated of the contents. The author used a series of nearly three hundred vibrating metal tongues, giving the tones through five octaves, and proceeding by intervals of two vibrations in the two lower, and of four vibrations in the three higher octaves. In the different series of experiments, certain of these tones were chosen as standards, and after sounding one of them for one second, a second tone, either the same or one differing from it by four, eight or twelve vibrations (higher or lower), was sounded at a variable interval, and the subject was required to say whether the second tone was the same or different from the first, and if different, whether higher or lower. Besides the answer could be "undecided," and also "different but undecided whether higher or lower." By adopting such a cumbrous method, and allowing the subject as many as five kinds of answers, Mr. Wolfe has very much diminished the value of his tables. For example, one of his strongest points is that we can tell whether two tones are equal more accurately than whether they are different. This does not at all follow from his tables. When the subject said "higher" or "lower," and was wrong, it may have been that the tones were really *different*, and thus the subject was only half wrong; *i. e.*, he recognized the difference, but not the direction of the difference. If we thus add the number of cases in which the direction of the change was recognized to the number in which the difference only was recognized, and estimate how many of the cases in which the direction of the change was misjudged, the fact of a change was recognized (on which point the tables are silent) as only one-half, it looks very much as though this statement did not hold. It is an excellent example of the mischievous effect of a poor method of experimenting or of stating one's results.

The general results of the paper, however, are probably not seriously vitiated by this inaccuracy, and may be summarized thus: The accuracy of the memory for tone sensations is very great; it is much more difficult to recognize the direction in which a tone has been altered than to detect the alteration itself. This seems to be a peculiarity of tone sensations, as it does not hold for sight or touch. The longer the interval between the sounding of the two tones, (variable from 1-30, 60, or 120 seconds), the smaller the chances of recognizing the tone; and this process of forgetting takes place at first very rapidly and then very slowly. It is made probable that the interval must increase in a geometrical ratio to produce an arithmetical series of (approximately) equal degrees of forgetting. A constant and peculiar deviation from this law occurs after an interval of 20-30 seconds; then there is a rhythm in the memory itself, and the curve of forgetfulness rises slightly. It was also noted that a low tone is not as easily recognized as a high one; that unmusical ears tend to judge low notes too low and high ones too high; that the effect of practice is at first marked, but soon diminishes, as is its general law; and, that the recovering power of the ear is so great that fatigue has little effect.

J. JASTROW.

The Conception of Love in some American Languages. By D. E. BRINTON. Proc. Am. Philos. Soc. December, 1885. pp. 536-62.

Dr. Brinton has studied the history and derivation of terms of affection as furnishing illustrations of the origin and growth of the sentiments of love and friendship; and has sought to show the parallelism that everywhere appears in the workings of the human mind. The principal words expressing love in the Aryan lanuguages can be traced back to two main ideas, one denoting similarity between the persons loving, the other denoting a wish or desire. The same notions underlie the majority of words expressing love in the American languages studied.

The following classification of the original modes of expression for conceptions of love is given, the names of the languages being given in parenthesis:

1.—Inarticulate cries of emotion, (Cree, Maya, Qquichua).
2.—Assertions of sameness or similarity, (Cree, Nahuatl, Tupi, Arawack).
3.—Assertions of conjunction or union, (Cree, Nahuatl, Maya).
4.—Assertions of a wish, desire or longing, (Cree, Cakchiquel, Qqueichua, Tupi).

W. H. BURNHAM.

Coma. By CHARLES MERCIER, M. D. Brain, Jan.,1887.

The writer, who avows himself a follower of Dr. Hughlings-Jackson, seeks to enforce Mr. Savory's proposition to restrict the present very vague meaning of coma to "cases where there is a state of insensibility from which the patient cannot be completely aroused, together with a tendency to death by asphyxia," except that for "insensibility" our author would substitute "evidence of defect of consciousness." This includes cases of partial consciousness and cases where consciousness may exist, but is not made evident by common tests. Four stages are distinguished. "The finest, most delicate and most elaborate movements and those associated with

the will are the first to go, while the simplest, broadest and most general movements, and those least associated with will are the last to be retained." Thus the drunkard looses the power to write first, then to talk clearly, then to hold his glass steady, then to walk, then to sit, and by the same law his breathing begins to fail, while his heart is unaffected, and the accessory parts of the breathing apparatus fail before the fundamental parts. Hence the tendency is to death by asphyxia.[1] Independent movement of the eyes, and especially divergence, is said to *always* occur in coma, and negative the possibility of hysteria. The ordinary fatigues of the day and a hearty meal check the most complex, delicate and precise movements of the mind. The same pathological event that enfeebles activity enfeebles mentation, and indeed every part of the organism. It is concluded that the highest centres represent in more or less degree every part of the organism. The functions of the brain are not "segregated in separated encapsulated portions of grey matter," and the doctrine of nerve centres "is rapidly becoming like so many doctrines before it, a fetishism." Obscure symptoms used to be called "reflex;" later they were due to "incoördination," a still more vague and sonorous expression of unusual cause, and now there are not only psychic, but trophic, and even glycogenic centres. To invent new centres *ad libitum* that may be both destroyed to account for defect and discharged to account for excessive action, shows how far localization has run mad. It belongs to lower and not to higher centres. The author's plea is for "universal representation of the highest nervous centres." This view assimulates coma to insanity as a "fulminating" form of it, and though the stages of insanity may be so prolonged that the relation of stages may be lost, both illustrate the one fundamental law of dissolution, and there is no form whatever of either that may not have its counterpart in a case of drunkenness.

Beiträge zur Kentniss der Militärpsychosen. W. SOMER. Allg. Zeitsch. f. Psychiatrie. 1886.

The peculiar psychoses resulting from the excitement and fatigue of military life and war have never been adequately studied. During active campaigning the medical staff of the army is otherwise employed, and save a few treatises on the simulation of diseases by soldiers and recruits, the literature on the subject is very meager. The basis of this article is mainly the clinical records of the lunatic asylum for soldiers in Allenberg, East Prussia, yet here diseases which developed after discharge from the army are ignored. The consequences of insanity in the service are very grave, and it is much more frequent than in civil life. Most soldiers are able-

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[1]See the conclusions of S. Weir Mitchell and E. T. Reichert in their very valuable "Researches upon the Venom of Poisonous Serpents," Smithsonian contribution, No. 647, 1886, p. 50. "These results all go to establish the conclusion that the respiratory centre is the most vulnerable part of the nervous system, that the coördinating and volitional centres are then prominently affected, that the sensory part of the spinal cord and sensory nerves are next attacked, and that the motor parts of the cord and the motor nerves are the last to succumb."

bodied, and between 20 and 25 years of age. From statistics gathered from various sources Somer concludes that the morbidity for psychic diseases is 0.027 per cent. for German infantry, 0.033 for Austria, 0.04 for France, 0.05 for Italy, and 0.16 for England. Long service in the colonies, involving fatigue in bad climates, are regarded as the cause of the high percentage for English soldiers. Psychic diseases are strikingly more frequent among officers than private soldiers. All these differences, however, between military and civil liabilities, are reduced almost to nothing in time of peace. The prevalent form of nervous disease resulting from war is paralysis, due to psychic and somatic exhaustion. Not only is war so deleterious in this respect that greater facility of exemption should be allowed those predisposed, but the prognosis of psychoses, due to active army service, is more unfavorable than for similar symptoms originating in civil life. In examining those entering the army, closest scrutiny should be given to the heredity and earlier life, with a view of reducing the too large percentage of military psychoses.

Die punctiförmig begrenze Reizung des Froschrückenmarkes. W. SIROTININ. Arch. f. Anat. u. Physiol. 1887. pp. 154.

It was known from careful series of investigations that when a stimulus is applied to the central end of the spinal cord, regular movements in the limbs are caused by means of nerves of deeper origin belonging to the lateral, and probably parts of the anterior tracts. Conversely stimulus of the lower end of cross-sections of the cord causes reflex movements by means of motor nerves from roots above. Finally it is known that a very brief stimulus of the ganglion column of the anterior horn causes strong and prolonged tetanizing effect in the nerves that originate here. The author, working under Ludwig's direction, devised the following ingenious and more exact method of extending our knowledge of the cord. The cord of the frog was well exposed from behind along most of its length. A sewing needle of smallest size was sharpened for three mm., with a lancet-formed blade, and of such size that the half of an average cord could afford room for ten thrusts, side by side. An average stab of one-tenth of a millimetre in depth would cut or disdisplace sixty fibres. The effects of these lesions were recorded on three muscles, the illeopsoas, semitendinosus and gastrocnemius. Of these the first was most sensitive from the second to the fourth vertebra, where its sensitiveness culminated and below which it rapidly declined. The second began to increase with the third and reached its maximum at the fifth vertebra, and the last reached a maximum of 100 per cent. at the seventh vertebra, where that of the first had sunk to 38. Almost the same law was observed, when, instead of sensitiveness, the heighth of the contraction of the muscles, or the order in time in which they began to contract, was observed. More complicated were the comparative results of the lateral stimulus of the posterior and anterior halves of the cord at different attitudes. Electrical stimuli were also applied with similar results. Incidentally an important observation was made that indicated a peculiar relation of the most outer part of the lateral column, the stimulus of which regularly affected the muscles of the same side, indicating that if the grouping is the same by mammals as

in the frog, these fibres cannot belong to pyramidal tracts. This result is incomplete and further results in the study of localization are expected by refinements of this method, and by applying it to the cord and perhaps even to the medulla of mammals.

Note on the Special Liability to loss of Nouns in Aphasia. By MARY PUTNAM JACOBI, M. D. Journal of Nervous and Mental Disease. N. Y., Feb., 1887.

From the record of one hundred and sixteen cases seventeen were found by the author to have lost only the memory or the power to employ nouns. Children are often said to learn nouns first, and they should therefore be most deeply organized, and, on the commom theory of devolution, the last to disappear. The records of autopsies shed no light on partial as distinct from total aphasia. Hence the author turns to the great discussions which have raged about the psychology of the naming process. Of course ideas are not held from the author's standpoint to have anything archetypal about them in the sense of Plato or the scholastic realists, but to be gradually formed by the fusion of visual, tactual and other impressions. For this product the terms *conception* and even *mental image* may be used by alienists so strictly as to realize the ever-lurking danger of realistic tendencies. The author agrees with Hughlings Jackson that a method which is founded on classifications which are partly anatomical and physiological, and partly psychological, confuses the real issues, and with Whitney that a word is simply the survival of the fittest among a variety of resources (gestures, etc.) for effecting the same purpose, viz.: fixing the mental attributes of an object, but prefers to use molecular and anatomical methods and terms, and considers that physiology on the whole favors nominalism. The author infers that the reason nouns are likely to be lost first and easiest in progressive aphasia is because they are most easily replaced by visual images, and adds in the last paragraph that it had been "suggested by a friend" that abstract nouns ought to be longest retained, and concludes that it would be interesting to test this suggestion. The suggestion has been made before, but not that we remember tested. If true, it does not seem to us sufficient to account for those strange cases of what Gairdner calls "brain intoxication for one word," at least not for those rare cases in which neither showing the object nor repeating the name will enable the patient to utter the name, where in Kussmaul's phrase the impressive as well as the expressive tract is interrupted. Is it not as possible that in the cases of those persons who forget or cannot speak their own names or that of their friends, or place of residence, but still use abstract and more recently acquired terms, the former have become more automatic or relegated to lower or more isolated centres, and are less widely irradiated by association, and so can be more cleanly eliminated by focal lesions. The author's treatment of the subject is at least broad and suggestive.

The Human Color-sense Considered as the Organic Response to Natural Stimuli. Journal of Ophthalmology. September, 1866.

Retinal Insensibility to Ultra-violet and Infra-Red Rays. Ibid. December, 1886. L. WEBSTER FOX, M. D. and GEO. M. GOULD, A. B.

The worship of sun, light and fire is the theological, the theory of either waves and specific energy of retinal fibres is the metaphysical stage in the study of light. But no study of phenomenon

is now complete without the psychological processes involved in knowing them are included, and it is by thus extending the scope of relations that the third or positive stage of knowledge is reached. Retinal processes in color-perception are at root fine perceptions of thermal differences. Under the theory of specific energy the mind's work was given the eye to do. Color is then, *in toto*, a psychological phenomenon. The reason why the so-called primary colors stand out so distinctly in the regularly differentiated spectrum is because first, gold, fire and light have always attracted great attention, and these rays are nearly half the whole. They symbolize reason. Secondly, green represents about one-fourth the rays, and stands for the vegetable world, which symbolizes utility and labor. Thirdly, red is war and love. It appears in blood and is associated with all its symbolisms. Fourthly, blue is the sky, remote, of feeble intensity, and typifying spiritual life, duty and religion.

In the second paper, it is urged that the limitations in the range of color-perception at both ends of the spectrum, and the coincidence of its intensity with the thermal intensity of the spectrum, is because the supply of infra red rays is weak and inconstant (as is shown by a reproduction of Langley's bolometric curves), and such power would indefinitely complicate the retinal and cerebral mechanism, and because finer discriminations within the imposed limits will be more useful to men. The authors write with a wide and suggestive range of reference and allusion to which justice cannot here be done, and one is often reminded of the "etherism" of the late Phillip Spiller. Both articles are disfigured by a number of misprints.

School-Training of the Insane. By J. G. KIERNAN, M. D., Alienist and Neurologist. October, 1886.

At an early period school-teaching was introduced into some American asylums. Thirty years ago Dr. Brigham thought great advantages had resulted from winter classes in the Utica Asylum. Writing, drawing, painting, mathematics and modern languages were taught, and even a journal was published by the inmates of a well-known asylum. It was thought to beguile the melancholy, occupy those who had recovered but lingered at the asylum for fear of a relapse, support those tending to dementia, and to help the convalescent. Need of fit mental occupation was felt to be one of the most pressing wants of insane hospitals. Dr. J. P. Gray and his school, starting from the correct premise that insanity was the expression of a physical disease, wrongly inferred that moral treatment was useless, and largely through their influence moral treatment fell into disuse. In many European asylums instruction, sometimes mental, sometimes by special teachers, was quite often found salutary for diversion and exercise, till under the influence of the extreme somatic school of Jacobi they declined everywhere, save in Ireland. Dr. Lalor's systematic plan, carried out in the Dublin Insane Hospital, where six regular teachers were employed, has met with wide approval, especially to relieve the gloomy monotony of county asylums. In many cases, especially those of *folie avec conscience*, vigorous healthy conceptions of an intelligent teacher or attendant, no doubt do tend to the recovery of patients, and the closer the contact the stronger the influence. Even Krafft-

Ebing approves Leuret's method of conquering insane conceptions by intimidation if arguments fail, and supplementing this system by school-training to change the current of thought and introduce new healthy ideas. A case is cited where a patient was cured of a tendency to repeat words and phrases by memorizing verses. The basis of this treatment is related to the principle that a shock to one's prejudice leaves the mind open to the influence of new ideas. The article closes with three interesting cases.

Muskelthätigkeit als Mass psychischer Thätigkeit. Forlaüfige Mittheilung. Von DR. J. LAUB. Pflüger's Archiv. 1886 [Dec].

The writer attempted to determine, by experiment, how much a given muscular action was reduced when a given psychic activity occurred at the same time. A maximal muscular clench was recorded on a dynamometer. Then after a rest the dynomometer was again taken in the hand, and some mental activity was begun, in the midst of which a maximal pressure on the dynamometer was again attempted, and found to be much less than before. The mental work done was mainly reading (so as to reproduce in substance), and mental multiplication of numbers of two figures each. The more intense the physic action the slighter is the contractive energy required to cause tremor. The relative effects of thought on the available power of the right or left hand respectively was also taken into account, and the whole study is subsumed under the principle of the constancy and equivalence of force. It has long been a desideratum in work of this kind to have a dynometer invented which can register fine differences of pressure when the absolute pressure is great, and, as Dr. Laub states he was engaged for a year on the problem of dynamometry, we may hope that when the full account of his work appears he will be found to have solved this problem, as well as to have overcome the manifold sources of error which will occur to physologists who read his preliminary statement.

Du Diagnostic Medico-Legal de la Pyromanie par l'examen indirect. E. M. DEMONTYEL. Archives de Neurologie. January, 1887.

In this long and valuable article, pyromania is limited to acts caused by irresistible impulse without sensory delusions or deliriums. Tenacity in denial, so different from the often prompt self-accusation of the impulsive homicide, who often feels the impulse and wishes to be restrained from its power, which the pyromaniac never does, may be due to the fact that pyromania chiefly occurs among the lower and feebler classes, whose favorite weapon is deceit. Thus direct examination of pyromaniacs is little to be relied on. Again, the presence of any motive is held to vitiate the claim of alienation as an excuse for such an act, though its absence does not establish it. The pyromaniac is rarely detected before having caused several conflagrations. Pyromaniacs are comparatively unknown in the city. Their acts are commonly done on sundays or holidays, or at the close of business hours. Very inflammable material which strongly suggests the approach of a match, especially tempts them. Thus occasion and probable security are dangerous. He does not fly, but is often the first to give alarm, and work devotedly to extinguish the flames. The disorder tends to appear at puberty and again in the climacteric. It is almost always attended by mental weakness. As pyromaniacs rarely incriminate themselves, it becomes the more important to study the many indications by which the diagnosis can be made, by indirect examinations. Six interesting new cases are described.

Ueber die Anwendung der Methode der Mittleren Abstufngen auf den Lichtsinn. DR. A. LEHMANN. Philos. Studen, 4 Heft. 1886.

The single careful study of this fourth psycho-physic method of average gradation made by Delboeuf, seemed to Dr. Lehmann, working under Wundt's guidance, inconclusive as to the validity of the method. Accordingly, he constructed three large disks of 10 cm. radius, each rotating independently by clockwork. One of these disks had of its four sectors the two alternate sectors black. By a pair of double sectors, one black and the other white, the shade of the other disks could be varied. The problem was to get the sectors of the middle disk, in a room uniformly lighted artificially, so that its shade seemed about midway between that of the other two. The first result showed that it made a great difference in judging whether the quantity of light from the middle disk was gradually increased or diminished. The conclusion of a long series of experiments was that the influence of contrast cannot be excluded, and vitiates the method of average gradation. Contrast could only be excluded by having a background for the middle disk that should always have the same degree of brightness as it has, changing with it, and the three disks must have such a distance between them as to exclude reciprocal contrast. Whether these difficulties can be overcome, can only be ascertained by further experiments.

Ueber die Theorie des Simultanen Contrasts von Helmholtz. E. HERING. Pflüger's Archiv. 1887. pp. 172.

Helmholtz's theory of simultaneous contrast seems to its author to have one of its strongest vouchers in the following experiment with colored shadows: Let a white surface be illuminated by a feeble ray of daylight and also by the reddish yellow light of a candle or gas jet, and let each cast a shadow upon this surface. The shadow of the first is yellow and that of the latter is blue, although it falls on a spot which receives only daylight. This is partly due to successive and partly to simultaneous contrast, and the former is readily eliminated. If the shadow cast by the gas is viewed through a tube so directed that the eye sees only a field within the gas-shadow, it does not seem blue, but does so if a part of the field lit by the gas is seen, adds Helmholtz. Hering, however, declares that this subjective blue is in no sense a "judgment," but is a regular phenomenon of successive contrast, and also that Helmholtz has made a similar error respecting the disappearance of the blue when the tube is laid aside. Hering proceeds to give an elaborate modification of this experiment in three phases, the description of which involves many pages, and which seems to show that the purely psychological explanation of this phenomenon should give place to his own physiological interpretation.

A Contribution to the Pathology of Dreams and of Hysterical Paralysis. By CH. FÉRÉ, M. D., of the Bicêtre Hospital, Paris. Brain. January, 1887.

It has been repeatedly observed that hallucinations that begin during sleep and are reproduced for several nights consecutively, end by being received as realities during the daytime. Diurnal delirium and even suicidal and homicidal impulses have been observed after two or three nights of dreaming. A single dream

sometimes sets up a mental disturbance that manifests itself the next day. A special relation between dreaming and alcoholic delirium has also been noticed. Dreams may play an important part as a determining cause of epileptic attacks. Nocturnal hysterical fits are sometimes determined by terrifying dreams. A remarkable case of psychic-paralysis due to the same terrifying dream for several successive nights is described. The same patient afforded also a good example of fatigue-paralysis, becoming incapable of phonation after the discharge of another centre. The author concludes that dreaming, and especially repeated dreaming, must not be considered an indifferent phenomenon, but may constitute the opening scene of a morbid drama. The reader would probably ask Dr. Féré whether such dreams as he describes were not caused by the paralysis, and not conversely as he assumes.

Onamatomania. CHARCOT and MAYMAN. Archiv. of Neurol. 1886.

A group of symptoms is designated where a single word plays an important role, often causing anxiety, and co-existing with habitual dubiations, fear of contact, or inverted sexual sensations, etc. (1) A single word or name may be irretrievably forgotton; (2) the patient may be impelled to continuously repeat a word; (3) in conversation certain words are emphasized; (4) certain words are used to check the effect of other accidental expressions; (5) as a word may seem to be accidentally swallowed, and great effort is made by hawking and spitting to bring it forth from the stomach. In such cases the patient has full consciousness of his state, and knows his enslavement to these tyranous impressions. Seven cases are described where the loss of a word caused great disquietude, and when it was found another was lost, and lists of words were made out and kept at hand for relief.

Experiments on Prehension. J. JACOBS. Mind, Jan., 1887. With Supplementary Notes on Prehension in Idiots by FRANCIS GALTON.

In these experiments the "span" of "prehension" is measured by the number of letters and numerals that can be correctly repeated after twice hearing, the interval between them in the dictation being about one-half a second. Ebbinghaus's nonsense syllables we at first tried, but rejected because they were found to distract attention and to be too variable in ease of pronunciation, rythm, degree of novelty and grotesqueness, etc. Numerals are not only fewer than letters, but have less associations by contiguity. Between the ages of eight and nineteen the span of school-girls increases from 6. to 7.9 for letters, and from 6.6 to 8.6 for numerals. Span increases not only with age, but with rank in class, and it is suggested that a "standard span" be added to the items for anthropometric measurement. Mr. Galton found greater individual variation in idiots, but less average span than in normal children.

Ueber Ziele und Wege der Volkerpsychologie. W. WUNDT. Philos. Studien. Heft 1. 1887.

The comprehensive program of Volks-psychology given by Lazarus and Steinthal, in the first volume of their journal makes it include language, religion, myth, customs, art and history, and contrast it with individual psychology. As descriptive natural history is illustrated by physics, chemistry and psychology, so history needs

a kind of natural history of mind, to which philologists and historians furnish raw material. H. Paul's division of all sciences into two classes, those of law and those of history, is less metaphysical. There is in fact no agreement what Volks-psychology, which is now separating itself from anthropology and ethnology, as these did from natural history, really is. Wundt thinks it should occupy itself exclusively with the three topics of *speech*, *myth* and *customs*, and as such, supplement individual psychology. Custom is the germ of law and shows primitive directions of the will; myth is the expression of living contents as conditioned by feelings and instinct; and language is their form, their laws of union.

The Science of Folk-Lore, with tables of the spirit basis of Belief and Custom. R. C. Temple. Folk-Lore Journal, September, 1886.

Folk-lore is defined as popular learning. The embodiment of popular ideas on all matters connected with man and his surroundings, or the popular explanation of observed facts. Its source is the instinct to account for such facts, and many customs have arisen therefrom. There is need of a standard manual showing just what kind of facts are wanted, and how they should be recorded and classified. The powers of imagination have been greatly overestimated. Its limits are conterminous with the bounds of human experience. Most of the customs of wild tribes, though coarse and strange, are sensible, and based on experience of what had stood them in greatest stead in the fight with disease and death. In conclusion, "demology" is suggested as a synonym of folk-lore, giving better derivitive forms, and a folk-lore library and museum, a better classification of proverbs, index of literature, a unification of the several discordant plans for studying it that have been put forth, are desiderated. The table is well calculated to show how many beliefs and customs are due to beliefs in spirits of many kinds.

Note sur un Charactère Différentiël des Écritures. J. Hericourt. Rev. Philos., May, 1887.

All movements of the hand from left to right are dextrogyric and those from right to left are sinistrogyric. Curves with their convexity upwward are centripetal, with the convexity below centrifugal. These designations may be used to characterize all movements, and, as Delaunay has shown, individuals and special groups of movements are characterized by the predominance of one or another of these traits. So in writing, dextrogyric may reduce, suppress, or even replace sinistrogyric curves, and each may be more or less exaggerated. In returning curves it is the first movement of the hand that is significant. The psychological interpretation of peculiarities of script, judged by these rubrics, is that dextrogyric writers, who not only in general stretch out letters rapidly toward the right, the direction of writing, but suppress sinistrogyric qualities, indicate superior psychic qualities. This conclusion is confirmed by experiments on hypnotic subjects under the influence of suggestion, illustrations of which are appended.

De l' intoxication professonnelle des dégustateurs de vins et de liqueurs. Dr. Donnet. An. Medico-Psychol. Jan., 1887.

As the symptoms lately grouped as tea-ism are sometimes produced both by drinking and tasting tea, so Dr. Donnet gives three cases of young men selected as tasters by the great dealers in wine

at Bordeaux, who were not drinkers, often swallowing only the best of the wines they applied to their lips, but who developed gastric and cerebral symptoms of chronic alcoholism, which were ameliorated on abandoning their duties as tasters. Wealthy people of Bordeaux are described as making wine a veritable cultus in the sense of having the most exqusite tastes for grades and varieties, having a special vocabulary for expressing faint nuances of tastes, and sometimes as consuming large quantities. Dr. Monache is made responsible for the statement that there are more deaths by apoplexy in Bordeaux than in any city of the world.

De la degustation des vins en Bourgogne. *E. Marandon de Montyel.* An. Med.-Psychol. Jan., 1887.

A broad distinction should be made between professional tasters, most of whom, in Bourgogne, do not swallow a drop of the wine they taste, and sometimes rinse the mouth with water, and amateur or occasional tasters who swallow, and who soon, and after a surprisingly small quantity, experience symptoms of intoxication. The former can pursue their vocation all day without inconvenience. Those who swallow sometimes lapse to chronic alocholism. The former sometimes acquire a disgust for most or all forms of alcohol. Those who have been wont to taste red wines and pass to white wines, often experience unfavorable effects. It is, however, tasters of tea and those kinds of white wine that need to be swallowed to be finely tasted who are most liable to professional intoxication.

L' encéphale, structure et description iconographiques du cerveau du cervelet et du bulbe. E. Gavoy.

The atlas part of this admirable work consists of fifty-five plates, drawn and reproduced by glyptography, of brain section only one millimeter apart, thirteen being sagittal, twenty-three being frontal, and the remainder horizontal. We are not told how the brains were prepared, the kind of cerebrotome used, nor the kind of bath in which the fresh sections were immersed. The fibres are much more distinctly brought out than in Professor Dalton's similar series of sections of frozen brains directly photographed. The labor involved in the work of M. Gavoy must have been great. The text is introduced by a general account of the nervous system, and comprises altogether over 150 pages.

Die Messung von Schallstärken. Starke. Philos. Studien Heft III. 1886.

By the aid of an ingenious mechanical device of Wundt, Starke showed that of two successive like sounds the second seems regularly greatest, perhaps on account of the rapid fading of the memory-image of the first, or perhaps by reason of the persistence of the first stimulus. This fact has entered as a source of error into nearly all previous measurements of sound. Eliminating this, the much doubted law of a simple proportion between the strength of sound and the product of height and weight is strictly valid, and thus Weber's law holds here within wide limits.

Der Sachverständige und der freie Willensbestimmung. SCHAFER. Viertlejahrsch, f. Gen. Med. N. F. XLV.

The author directs a vigorous polemic against Mendel, who holds that the diagnosis of insanity involves irresponsibility. Schaefer claims that such diagnosis *in foro* does not suffice, because insanity in a scientific and in a juristic sense by no means coincide, and a patient may be insane and at the same time responsible. Only the physician can decide at what point responsibility ceases, and this is not involved in a verdict of insanity. The doctor does not pass beyond his sphere in investigating the general psychological idea of free will.

Eine Karte des Menschlichen Auges. W. FLEMMING. Braunschweig, 1887.

This chart is a very careful delineation in colors of a vertical section of the human eye thirty times enlarged. The normal proportion of parts and dimensions is preserved so far as the scale permits, and we have already found it of great pedagogical utility in the class-room. It is accompanied by a pamphlet of explanatory text.

NOTES.

A permanent international ornithological committee, under the patronage of the crown prince Rudolf of Austria, has already begun the systematic collections of data on bird-psychology, including nest-building, brooding, feeding, uses and injuries, relations to their friends and foes, habits known to hunters, popular names, but more especially their migrations. A special journal, *Ornis*, was begun in 1885, and is largely devoted to the publication of these data. Observations from eighty-three stations are reported in systematic geographic order, which so far include three hundred and fourteen species of birds. Two annual reports (Jahresberichte) of naked observations have been published.

The Medico-Psychological Association of Great Britain and Ireland have instituted examinations for "certificates in psychological medicine." These examinations were based on three months' experience in asylum work, or lectures for a like period, and are both oral and written. Only physicians were admitted, and its certificates were sought not only by those desiring lunacy appointments, but as guarantees of the fitness of practitioners to deal with mental cases and sign medical certificates. The Gaskell memorial fund has since been applied to a prize for excellence in these examinations. It amounts to one thousand pounds, and two years' residence at an asylum is now required. The subjects for the annual honors examination, are four: 1. Healthy and morbid histology of the brain and cord. 2. Clinical cases, with commentaries. 3. Psychology, including the senses, intellect, emotions and volition. 4. Diagnosis, prognosis, pathology, treatment and medico-legal relations of mental diseases.

The Senate of the London University introduced for the first time in place of the old examinations in logic and psychology in the M. D. course, the subject of "mental physiology especially in relation to mental disorders." In 1886, the old subjects were also allowed as an alternative, but in future they will disappear. We cannot agree with the Journal of Mental Science that the examinations should be made to conform with that of the Medico-Psychological Association, but hold conversely that normal mental physiology should form one of the bases for the study of mental disorders.

The committee having the Belhomme prize in charge, consisting of M. M. Bouchereau, Dagonet, Fèrè, Foville and Sèglas, announce the following subject: "Investigation on the question whether there exists any emotional, physiological or psychological signs peculiar to criminals." Manuscripts must be in by the last of December, 1888.

Among the useful translations published in whole or in part in *The Platonist* since January, 1887, when the journal, now in its third volume assumed a new and better form, are the commentary

of Proklos on the first Alkibiades, Iamblichos on the Mysteries, Synesios on the Philosopher's Stone, the eleventh book of the Metamorphosis of Apuleius, the life of Hai Ebu Yokdan. The April number contains a plea for the establishment of a school of philosophy, chiefly to be devoted to esoteric theosophy, to be located at Los Angelos, California.

Archiv. fur Geschichte der Philosophie is the title of a new quarterly to be edited by Professors E. Zeller, L. Stein, H. Diels, Dilthey, and B. Erdmann. The history of philosophy has hitherto had no proper organ. Articles on these subjects must be sought in philological and theological, as well as philiosophical reviews. These are to be united in the new review, and speculative articles will be excluded. Contributors may write in German, Latin, Italian, French or English. About half of each number of the review will be devoted to notices of current publications bearing on the history of philosophy. Each number will contain about 150 pages. All communications should be addressed to E. Zeller, 4 Magdeburger Strasse, Berlin, or to L. Stein, Universität, Zürich.

In a brief article in the Journal of Nervous and Mental Diseases, December, 1886, Dr. Spitzka, concludes from the study of a cat which had been kept alive three months after the destruction of its entire left cerebral hemisphere and thalamus, that there is "a system of fibres intermediate in position between the pyramid and interolivary tract, and decussating with the former, apparently derived from the neuclei of the posterior columns, and running with the latter on its cephalic course. It was probably the discovery of similar fibres that led Meynert into the formation of his well-known but now abandoned view, regarding the sensory and motor decussation of the pyramids." This would prove that these tracts contain fibre admixtures from other sources than those from which Flechsig exclusively derived them.

During the twenty-four years ending 1887, Dr. Allen Starr, of New York, found 160 cases of cerebellar disease reported in American literature, in 40 of which symptoms and autopsies were described sufficiently well to warrant conclusions. In these there was: headache in 26; insubordination in 25; vertigo in 20; vomiting in 18; blindness in 14; dim vision in 6; diplopia or strabismus in 7; deafness in 7; hemiplegia in 9; mental symptons in 8; partial paralyses in 4; facial spasm or parerves in 4; stupor in 7; convulsions in 1; mania in 1; sexual excitement in 2. Four interesting cases are described in detail in the same journal, (The Journal of Nervous and Mental Diseases, April, 1887), by Dr. Seguin.

J. H. Lloyd, in a thesis for admission into the American Neurological Association, published in a late number of the Journal of Nervous and Mental Diseases, after a long exposition of the mischief of metaphysical abstraction in the study of mental phenomenon, concludes that the doctrine of moral insanity implies that there is a distinct moral faculty in the sense of a distinct agent which may become diseased without at all affecting the health of other faculties. It, and a big brood of special manias, is but the creation of bad science. He does not add that the "faculty" is also very differently understood according as the utilitarian or intuitional view is adopted, which is another argument against it.

Dr. R. L. Parsons, in the Journal of Nervous and Mental Diseases, April, 1887, summarizes the objections that have been so often put forth against the term monomania, and recounts the different senses in which it has been used since it was first introduced by Esqnirol. The term has been but slightly used of late in hospital reports in this country, and with little uniformity, because its literal and scientific meanings are so at variance. It is especially inconvenient where often used, viz.: in courts of justice. Partial mania and paranoia, which have been lately used in its place, are also unsatisfactory, and the term oligomania is proposed as a term whose obvious etymology best agrees with clinical facts.

Dr. Francis X. Dercum, in an article entitled "Facts and deductions bearing on the action of the nervous system," in a late number of the Journal of Nervous and Mental Diseases, concludes that considerations of embryology and comparative anatomy, and various facts derived from other sources, point to the conclusion that the nervous system, though inextricably complex, and composed of an almost infinite number of parts, acts as a whole, and that its parts are so closely related and interdependent that no one part can move unless every other part, no matter how slightly or how profoundly, moves also.

Dr. M. Allen Starr, in the Journal of Nervous and Mental Diseases for February, 1887, urges the more general employment of charts, like those of Erb, of electrical reaction, especially in paralytic and allied disorders. The author even thinks that in recoverable cases these curves enable prognoses as to the date of recovery to be quite exact. A simple chart is given for illustration, made by the aid of an absolute galvanometer in Milliamperes. The too common method of basing comparison on the number of cells is fallacious, as their strength is too variable.

Dr. Orgeas, in his book entitled *La Pathologie des Races Humaines et la Probleme de la Colonisation*, has rendered a real service to anthropologists and sociologists. A vast variety of facts is passed in review to illustrate the biologic principle of the non-cosmopolitanism of man. Permanent changes of latitude and climate are not permitted without deterioration. A change of residence brings artificial conditions of life. The physical differences in men are due to adaptation to different environments. The comparative pathology of different races is passed in review, and an important role is ascribed to it in determining the main facts of history. What races can best be adapted to what alien climate and by what adjustments this adaptation can be made with least loss, is fast becoming a grave problem in European statesmanship.

Dr. Gellé prints in *L'Encaphale*, 1887, No. 1, an interesting abstract of some observations made by himself on the role of the sensibility of the tympanum in the orientation of sound. A subject of Charcot's, on whom the first observation was made, was afflicted with general anaesthesia of the skin, extending to the external meatus of the ear and to the tympani of both ears, which were absolutely insensitive to contact or to pain, while light and hearing were intact, and the patient preserved his intelligence entire. With the eyes closed he could hear well the tick of a watch, but found it impossible to tell on which side or in which ear.

The experiments were repeated on this and other subjects suffering from general dermal anaesthesia, and always with the same result. Hence it is inferred that the sensibility of the tympanum is stimulated by the vibrating current, and hence comes not only the sense of direction, but of exteriority.

A recent visit to Gheel is reported in the Journal of Mental Science by Dr. Hack Tuke. The town now contains about 3,000 houses, about one-third of which receive lunatics, of which, including a few in a small asylum recently built for those of dirty habits, there are now over 1,600. The colony is under the general control of seven commissioners, who meet quarterly and report to the minister of justices, and is divided into two wards, each under a physician. Some of the cottages where the poorer patients are taken in and boarded are very humble. In the latter houses, where several patients are boarded out, is a special attendant. The highest sum paid is 200 pounds per year. The place is not very cleanly, and police regulations do not prevent the patients from entering public houses where drink is sold. The patients are mostly employed in some sort of labor, and offenses against morality are rare. There has been no homicides since 1850, and no fires set by patients for many years. Suicides have been rare, and there have been but three or four illegitimate births for the past ten years. Only sixty patients are in the asylum itself, which was opened only in 1861. The town was famous as a resort of lunatics in the fourteenth century. The church of St. Dymphna, built in that century, still contains the sick chamber where spiritual treatment was administered to lunatics. An iron chair, attached to a bedstead for restraining patients during the night, and iron rings are in the floor near the fire-place, to which the patient's chair, if not the patient, is secured. Dr. Tuke and his party were impressed with the economy and the moral effects of this system. The success of it cannot be judged by the "Scotch system" at Kennoway in Fifeshire, which is in many respects modeled after it. The latter is far smaller, is made up mostly of chronic dements, but the patients are more widely scattered and more intelligently supervised. The writer concludes that on the whole the plan is not desirable on any large scale in England, among other things, for the detriment it involves to the domestic life of the family which takes charge of the lunatic.

Dr. Francis Warner's "*Physical Expression*," vol. 52, of the international science series, is an original work of much value to the psychologist. The study of motor functions seems more likely to the author to lead to practical results than the subjective process of interpretation of feelings. People fitted for self-analysis are few and peculiar, so that only the small part of the field representing this coincident peculiarity has been worked over by this method. His method for screening different parts of the face is to cover the adjacent parts with a sheet of paper. Irritability in children causes the head to rotate, expressing negation, and drill in flexion, or nodding the head induces a state of acquiescence. The relation of hand postures to diseased states, and how to get the expression of mental action long before speech can disclose it, are points yet to be studied. The study of physiognomy apart from the rest of the body is an error.

At a meeting of the *Société de Psychologie Physiologique* Feb. 28, 1887. M. Babinski presented the results of experiments on an anæsthesic subject illustrating the survival of knowledge of the limbs and knowledge of movements accomplished after all dermal sensibility had been totally lost. With the eyes of the patient bandaged the arms could be placed in any position without any knowledge on his part of a change of place. If his eyes were closed while his hands rested on his knee, they could be lifted above his head afterwards and he believed they still rested on his knee. He was very clumsy and things slipped from his hands. With an arm elevated by a weight and pully, and being told to touch his knee, he felt for it about his shoulders. The time taken to affect movements and modifications of respiration seems the only basis of such rudamentary judgments of the position of his limbs as still remained.

Among the conclusions drawn by Dr. Descourtis, in a recent article on the cerebral thermometer, are the following: Sometimes the temperature reaches its maximum in fifteen or twenty minutes, and sometimes three or four hours are required; sometimes the temperature itself, or its slow increase or decrease, is constant, but often presents oscilliations which are commonly not regular, and sudden falls without known cause often occur. The temperature of the two sides often varies independently, and this difference seems greatest at low temperature, and sometimes it may rise on one side and fall on the other.

In an extended series of experiments on the mental representation of space, in connection with the feeling of effort, in the Rivista di Filosofia Scientifica, Mar-Dec. 1886, E. Morselli reproduced lines of various lengths with eyes open and closed, from which he concludes that the pshcho-physic law of the distribution of particular values about a mean hold of representations of space; that the tendency to increase small and diminish large distances holds of space as of time; and that the loss of spacial representation after a time illustrates the loss of memory, and proves that the concept of space is a product of habit and motor experience.

Dr. Paul Richer's Étude sur la Grande Hystérie ou Hystero-Epilepsie, is a work of great value to the psychologist, which, however, appeared too early for extended review in these pages, as it reached its second and greatly revised edition in 1885. Like Charcot, he ridicules the "pretendedly scientific skeptician," which ignores or even doubts the existence of the strange phenomenon here systematically described. Patients subject to these attacks were more common in France than elswhere, but it will be interesting to know if it may not also be observed in the Welsh and Irish branches of the Celtic Family. The stages to the description of which the work is mainly devoted are: (1) prodromata; (2) epileptic with tonic and clonic cramp, and relaxations or resolution; (3) contortions or clownism; (4) emotional and passionate attitudes; (5) delirium and hallucinations.

Dr. I. N. Ramaer, inspector of asylums in Holland, in an article on psychial analysis as the basis of morbid psychological diagnosis, urges that a careful study of the facts and laws of normal must

precede that of morbid psychology. The author starts with consciousness and thinks its origin is in the grey matter of the floor of the fourth ventricle, beneath the median groove. The just and important thesis that psychiatrists need psychology is not very adequately represented.

Bianchi (Arch. Ital. per le Mal. Nerv.) reports a grave case of hysteria, with hallucinations of hearing and visions, completely cured by a drastic course of moral treatment, which consisted in threatening an ovarian surgical operation with an elaborate display of apparatus, in applying a pretended cautery, really cold, to the abdomen, and in compelling the patient to appear in public at the moment of an attack.

Lussana (Arch. Ital. per le Mal. Nerv.) reports experiments on a surface of the outer part of the leg of a female patient of forty-five, who had lost the skin over a surface of 10 x 12 centimetres, from which he concludes that discriminative dermal sensibility is mediated by papilary bodies, particularly Meisner's corpuscles, but that the sense of material contact is independent of these bodies. The destruction of the skin and its nerves has no influence on the muscular sensibility of subjacent contractile bodies.

Vega (Arch. Ital. per le Mal. Nerv.) reports an interesting case of *folie a quatre*. The father of a woman of forty-seven was a foundling, who devoted much time and labor to find his true parents. She talked much of this, and at length thought herself the daughter and heir of high personages. This conviction she instilled into her mother (2) and into a lady whose servant she was, (3) and who identified a rich general as the father of her femme de chambre. The convictions persisted despite all disproofs, and when the author of the delusion married, her husband soon became infected with the monomania. The four had a course of similar hallucinations of hearing and general sensibility, erroneous interpretations, grievances, etc., and even after separation the deliriums continued, taking no account of the death of their assumed protectors and persecutors.

Algeri (Arch. Ital. per le Mal. Nerv.) reports with tables observations on 314 insane women from fifteen to forty-five, from which he concludes that the menstruration is almost always more irregular with insane than with sane women. In chronic psychopathies and in dementia menstrual disturbances are more marked than in melancholy and mania of recent origin. Menstruration generally coincides with psychopathic aggravation. This relation is best seen in periodic insanity.

Guicciardi and Tanzi (Rev. Sperim. di Fren.) tested the reaction time of fourteen cases of chronic hallucination of hearing with systematized delusion, and found it to be on the average 117.5 thousandths of a second, as compared with 119.5 in ten normal cases.

Tanzi and Riva (Rivista Sperim. di Fren.) observed 103 cases of systematized delusions among 729 insane patients, and infer that this psychophysic modality is less frequent than might be inferred from the prominent place it occupies in the body of psychatry. Women are less liable to it than men, and it develops especially at the

menopause. It is eminently a malady of degeneration, chronic, and of very long evolution. It is neither uniform in its progress nor is its form fixed, but its metamorphoses are extremely gradual. It is a perversion not an enfeeblement of the faculty of thought.

Amadei and Tonnini (Arch. Ital. per le Mal. Nerv.) have prepared the following classification of systematized delusions or paranoia. I. Degeneration paranora, on the basis of a congentially vicious organization, with somatic signs of hereditary degeneracy. Its development may be a. early, or b. late, and each of these may be (*a*) simple, *i. e.* with delirium of persecution, ambition, religion, love. On (*b*) hallucinnatory, *i. e.* the same as (*a*) with hypochondriacal symptoms. II. Psycho-neurotic paranoia. A. primary. B. secondary. Each with sub-divisions.

Baistrocchi (Rev. Sperim. di Fren.) has determined the weight of the white and grey substances of the brain by the aid of a Nicholson monumental aerometer in air and in distilled waters. The density of the entire encephalon in 21 men was 1.0265, in twenty-two women, 1.0338; of the spinal cord, 1.0387 for men, and 1.0448 for women. The grey substance of the cortex is lightest. Next comes the white cortical substance, the grey of the basal ganglia, the cord, the mid-brain and cerebellum which is heaviest of all. In general, density increases with absolute weight. It increases to the age of forty, and then steadily decreases, while the cord is at its maximal density in the fœtus.

Tamburini and Riva (Riv. Sperim. di Fren.) in sixty cases of general paralysis found lesions of the frontal lobes in 56 cases, of the parietal in 44, of the sphenoidal in 27, the temporal in 19, the occipital in 9, and the island in 3.

Morselli (Riv. Sperm. di Fren.) has applied the dynamograph of Régnier with registering apparatus to the diagnostic study of nervous disorders in diseases of the nervous system. The normal curve of the healthy subject is not very unlike that obtained from an excised nerve-muscle preparation; the neuropathic curves present many variations which are highly suggestive, but we opine, not sufficiently studied to present settled results as yet.

Musso (Rev. Sperm. di Fren.) examined the pupils of 70 epileptics and found them no larger than normal individuals. The diameter in sanity oscilates between 3 and 6 mm., with epileptics from 2 to 6 mm. The prodromal stage of convulsions is often signalized by remarkable difference between the diameter of the two pupils.

Adriani (Rev. Sperm. di Fren.) argues that the doctor should give the same care to the school that a mother gives to her child. Children with hereditary predisposition to insanity or general neural weakness, should not be educated in schools with perfectly normal children, but apart in special institutions, as they need prophylactic treatment.

A remarkable case of hereditary colored vision is described in The Rev. Philos. for Feb., 1887, in which a father son and daughter saw vowels and consonants similarly colored.

Liébeault, in his *Confessions d' un Medicin hypnotiseur*, recounts the different methods of producing hypnotism that he has used. Braid's fixation method was found occasionally to cause convulsions, and he recommends *gradual* suggestion, drooping of lids, falling of head, heaviness of lids, etc. Gradual awakening was also found far less productive of unpleasant sensations than if sudden.

Fioretti, in a late number of the Archivio di Psichiatria Scienze Penali, attempts to show that in normal, pathological and hypnotic cases the given and conscious motive is not the real cause of the act, but a mode by which the agent indulges the causal instinct, by explaining to himself conduct the primal source of which has escaped him. Thus the motive for a crime is not the absolute criterion of imputability.

Lombroso recounts some remarkable cases of memory of letters and figures impressed hypnotically, and enumerates the pernicious results of the exhibitions of the public mesmerist Donato at Turin alone as follows: One of his subjects was soon after attacked with paresis, another while at a theatre became cataliptic, another thought himself always hypnotized, another fell into epiliptic convulsions; a mathematical student could not adjust his compass without becoming cataleptic, and another must run after all carriages in the street with lanterns. All such public exhibitions, it is concluded, should be forbidden.

Danilewsky, in a late number of the Archives Slaves de Biologie, reached experimentally the conclusion that the stimulating influence which the hemispheres exert on the optic lobes, bulb and medulla is replaced, after ablation of the brain by increased excitability of these organs caused by excitations from the external world through the senses.

Jendrassik in two recent articles in the Archives de Neurologie, rejects the chemical vaso-motor and all other current views, and concludes that the cause of hypnotic phenomenon is loss or diminution of association.

Cappie, in Brain, July 1886, concludes that in attention the encephalic circulation is concentrated in certain cells, and as the quantity of blood in the brain cannot be increased or diminished other parts of the brain are depleted, In epileptic attacks, moter cells absorb the blood, leaving the cells in which consciousness subsists anaemic.

Ch. Richet, (Rev. Philos., Jan., 1887), describes the typography of certain neuropathic subjects which gets into print, and which, like their writing, is often marked by the most bizarre traits. In twelve lines eleven are different kinds of type; seventeen lines are from twelve different fonts, etc., and compares with this the style of a "certain contemporary school of poetry, the lines of which may be read in inverse order, so profoundly obscure is the sense.

In an experiment made on hysterical subjects in different states of hypnotism and echolalia, and lately reported to the French Society of Physiological Psychology, the reaction time from ear to mouth in the waking state was thirty-nine hundredths of a second; in somnambulism thirty-three, and in the state of echolalia, hypnotically induced, but thirty-one.

Peli compares the cephalometry of 670 lunatics with sane persons of the same class and locality, and finds the insane head is longer, higher and broader, and anomolies in the shape of the skull are three times as frequent, more so in males than in females, and mostly in hereditary forms.

Poggi examined the cerebral convolution of fifty brains of the insane, and found anomalies more common than in the sane, especially in the left hemisphere. The most frequent anomaly is a double calcarine fissure, or communication between the internal occipito-parietal sulcus and the sulci of the cuneiform lobule—40 per cent. Insane brains are particularly characterized by numerous anastomotic folds.

Dr. Legrain, in his Du Délire chez les Dégénérés, would substitute the term degenerative for hereditary insanity. He describes the physical and mental symptoms characterizing such cases, and describes them as very slowly evolving, taking on different forms sometimes in rapid succession, recovery from one followed by relapse to another, often attended by alcoholism and ending in dementia.

H. Beannis reports in a note in the Rev. Philos., March, 1887, the following experiment: The sensibility of the mucous membrane of the vocal cords of a singer was destroyed by cocaine without sensibly altering the accuracy of his song. From this he concludes that there is a muscle-sense which plays its role in giving accuracy to notes.

In a late number of the Medical Times Dr. L. W. Fox and G. M. Gould plead for a law restraining peddlers, jewellers and opticians from prescribing glasses. Many cases of injury, often permanent, to eyes resulting from errors thus made are cited, and it is claimed that legal restrictions are as much needed for the optician as for the druggist.

Bertillon proposes to identify criminals by measuring their height, the head, length of left arm and foot, and colors of the right eye. Altogether these measurements have already been the means of recognizing over seven hundred rearrested criminals without a single error.

Jeronimo Vida considers society an organism, the individuals composing which are related as cells in the animal body, and would thus approximate social and biological sciences. The social individual, however, is not the single person, which, unlike the physiological cell, lacks the reproductive function. This idea has been long ago worked out in far greater detail by Linienfeld, Espinas and others.

Dr. Christian argues, in a late number of the Journal of Mental Science, that general paralysis does not entail any increased fragility of the bones, and that osteo-malakia, when present, is the result of other causes. The great number of fractures in these cases is due to the great number of falls.

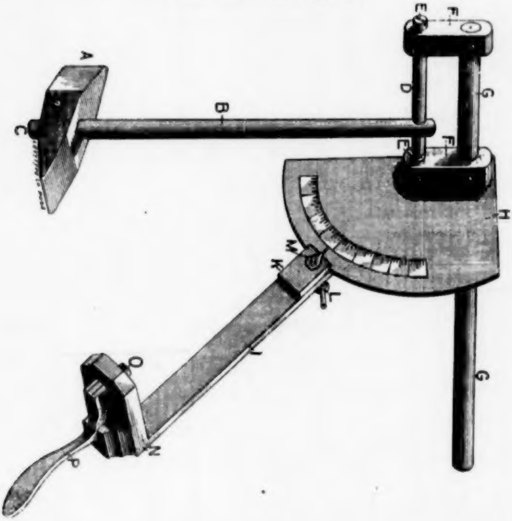
Dr. C. W. Cobbold prints, in the Journal of Mental Science, an elaborate plan, with cuts, of a model lunatic asylum for three or four hundred patients, with an interesting discussion of other plans.

Montegagga's *La Physionomie et les Sentiments* is not only a scientific but a most readable book. The cuts, by Hector Ximenès, are a new departure from the wearisome reproductions of Lavater, and the face is discussed feature by feature, in a way which marks an advance beyond both Darwin and Delsarte.

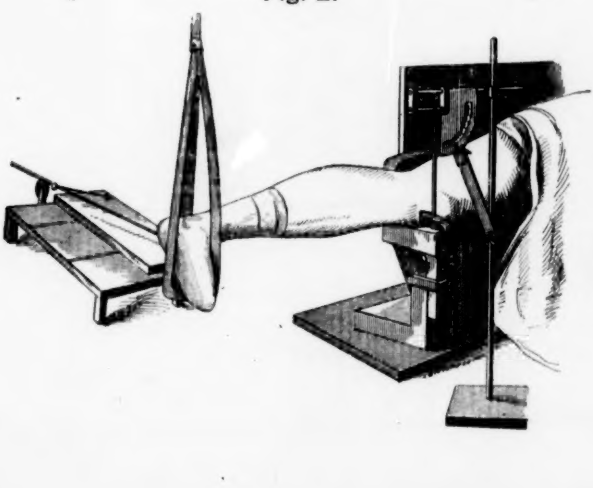
J. Heiberg's *Cutaneous Nerve Supply*, translated into English by Wagstaffe, is a small, useful book for both students and practitioners. The plates are a judicious compromise between plainness and literal reproductions from nature.

Lengelmann's *Idiotophilus* is a new, valuable and comprehensive work on idiocy by an eminent specialist. It is on the whole the best general treatise on the subject since Seguin.

PLATE I.—Fig. 1.



a Fig. 2. *b*



EFFECT OF ALL THE DIFFERENT

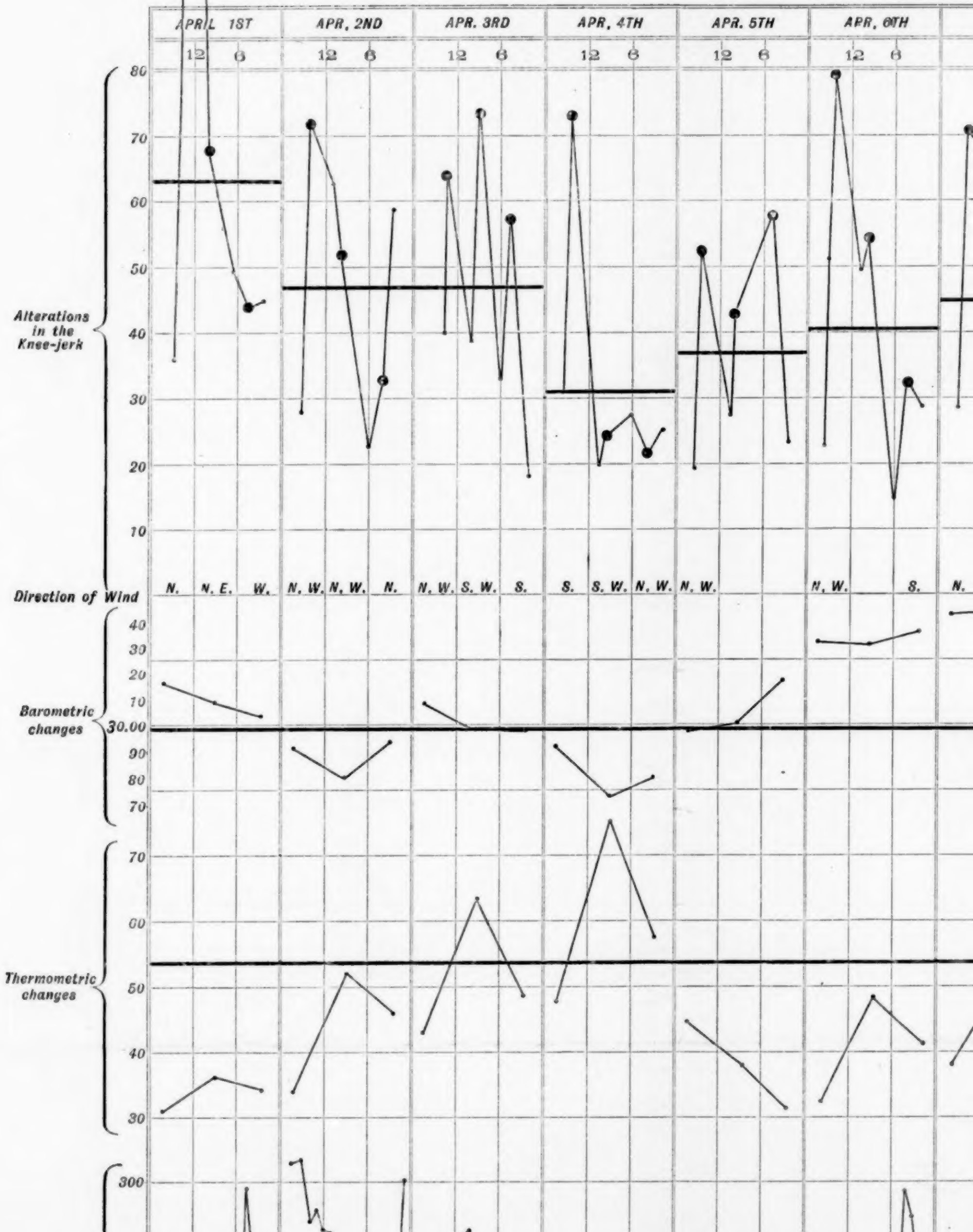
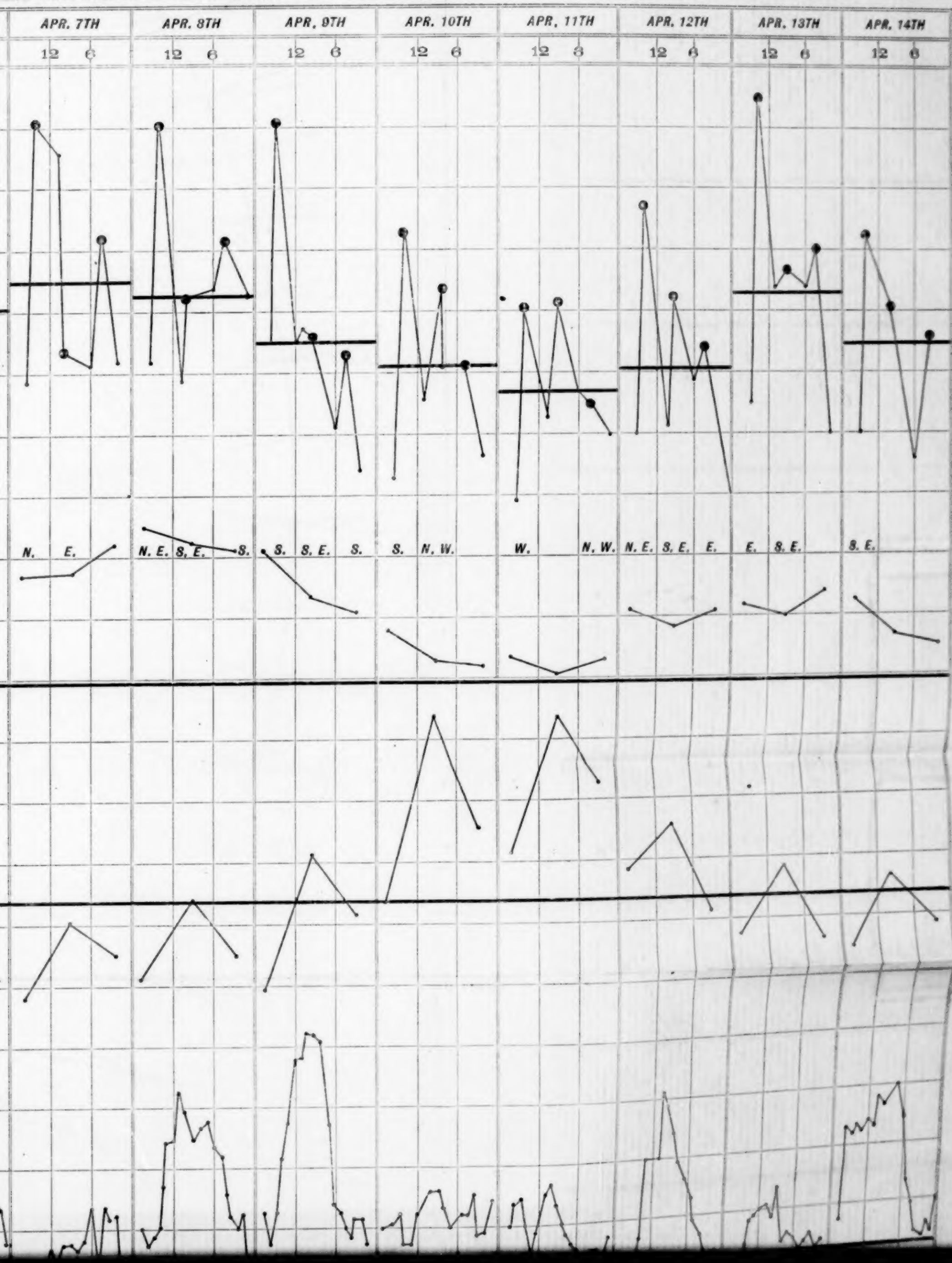


PLATE II.

ENT COMPONENTS OF THE WEATHER UPON THE KNEE-JERK



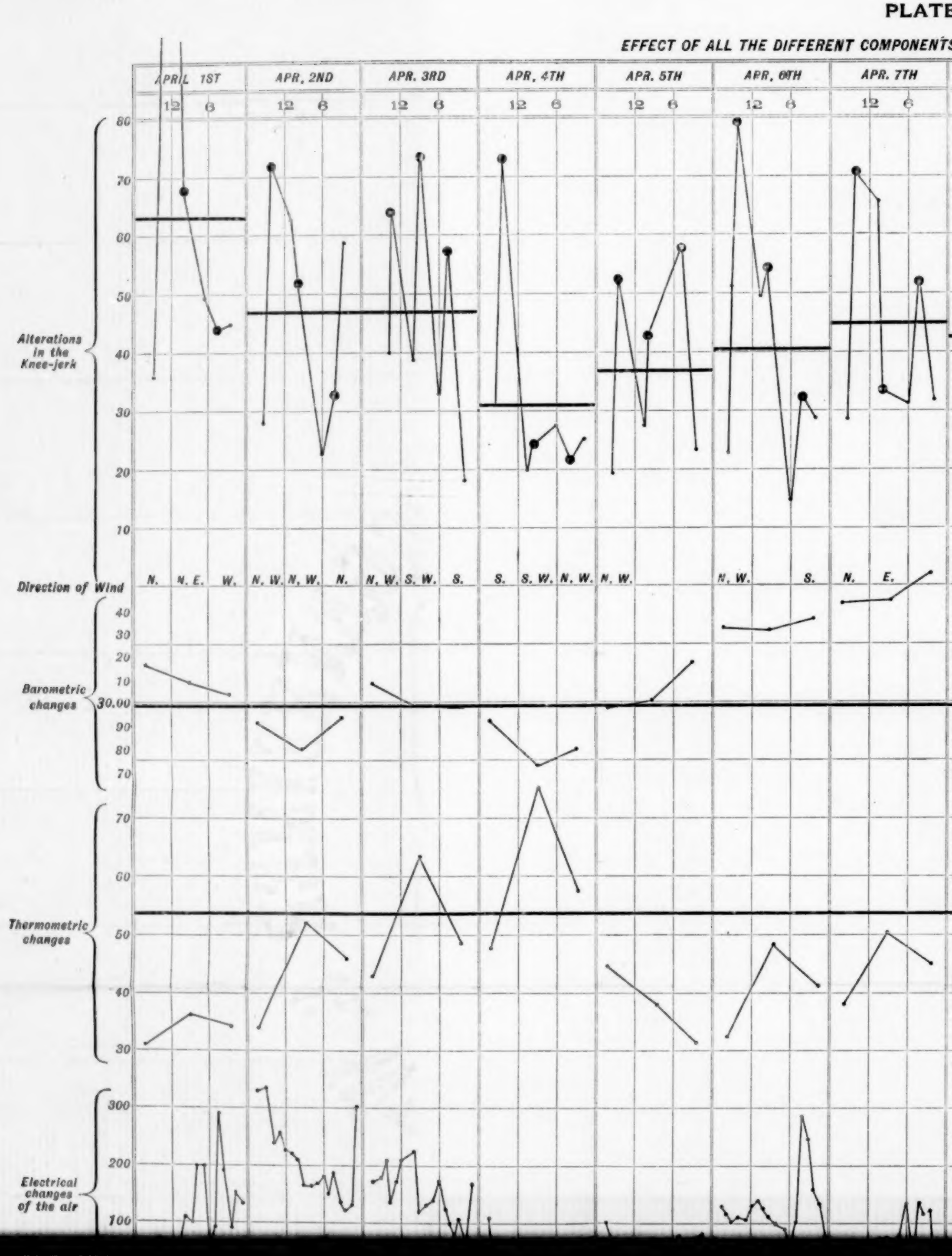
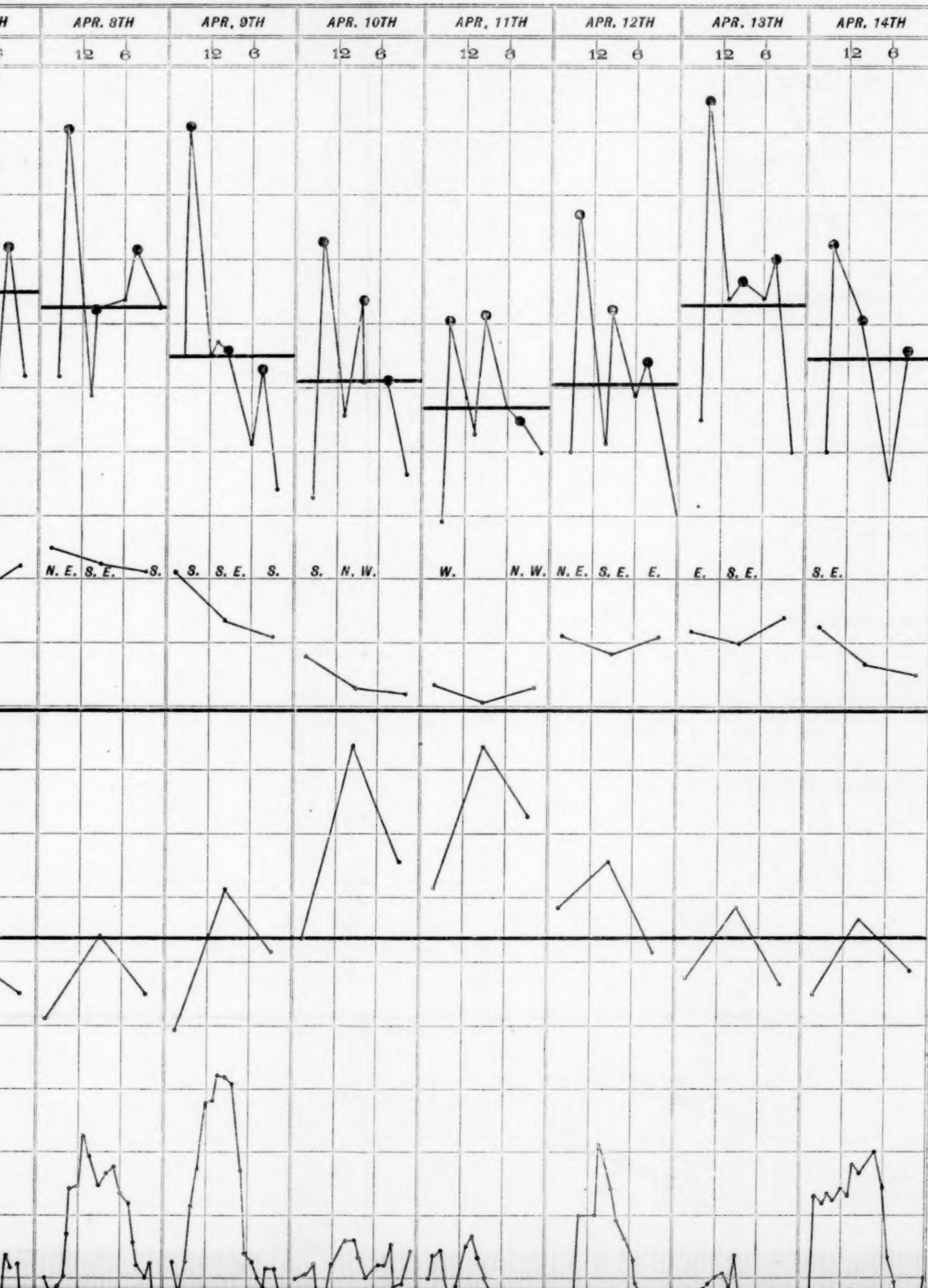
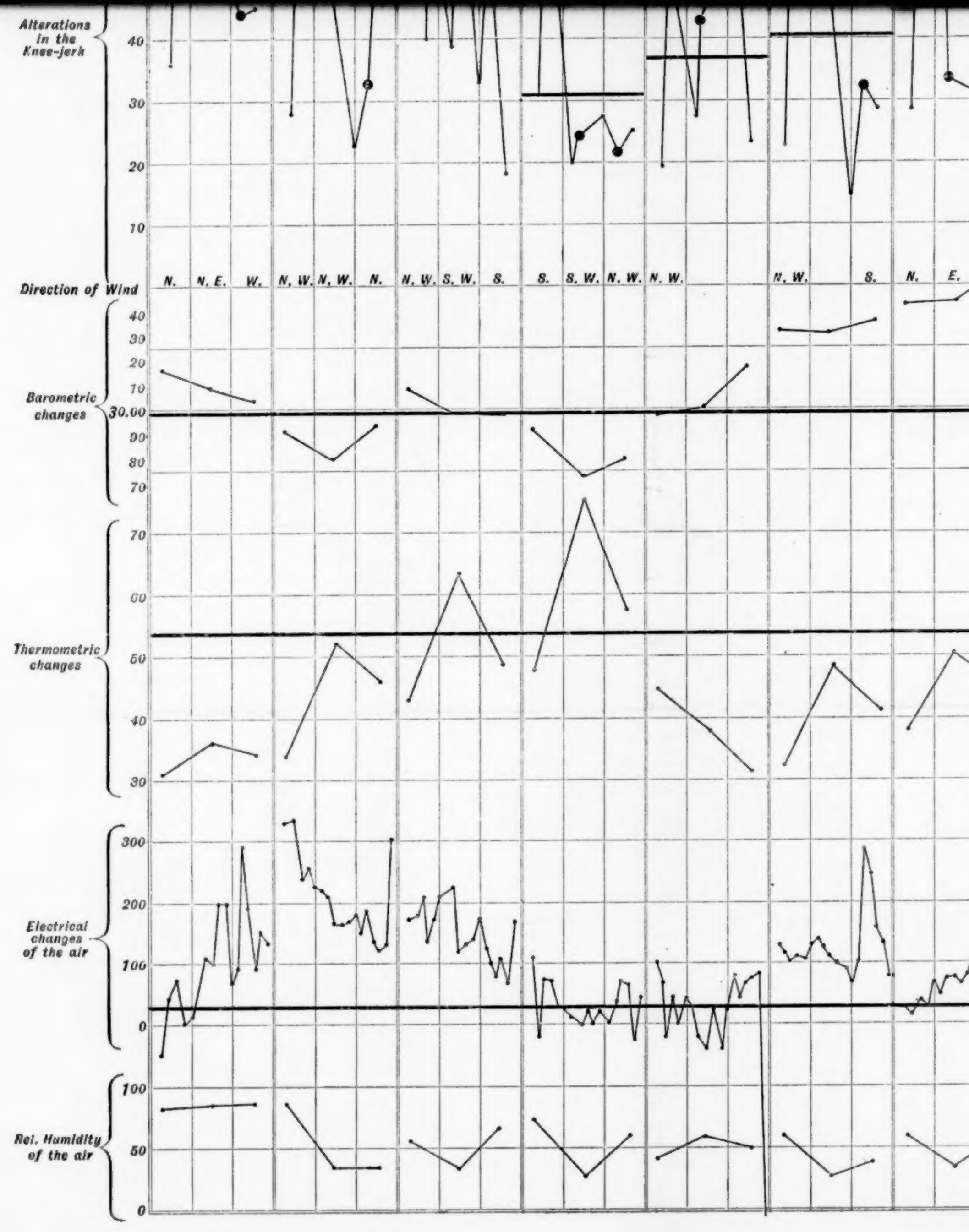


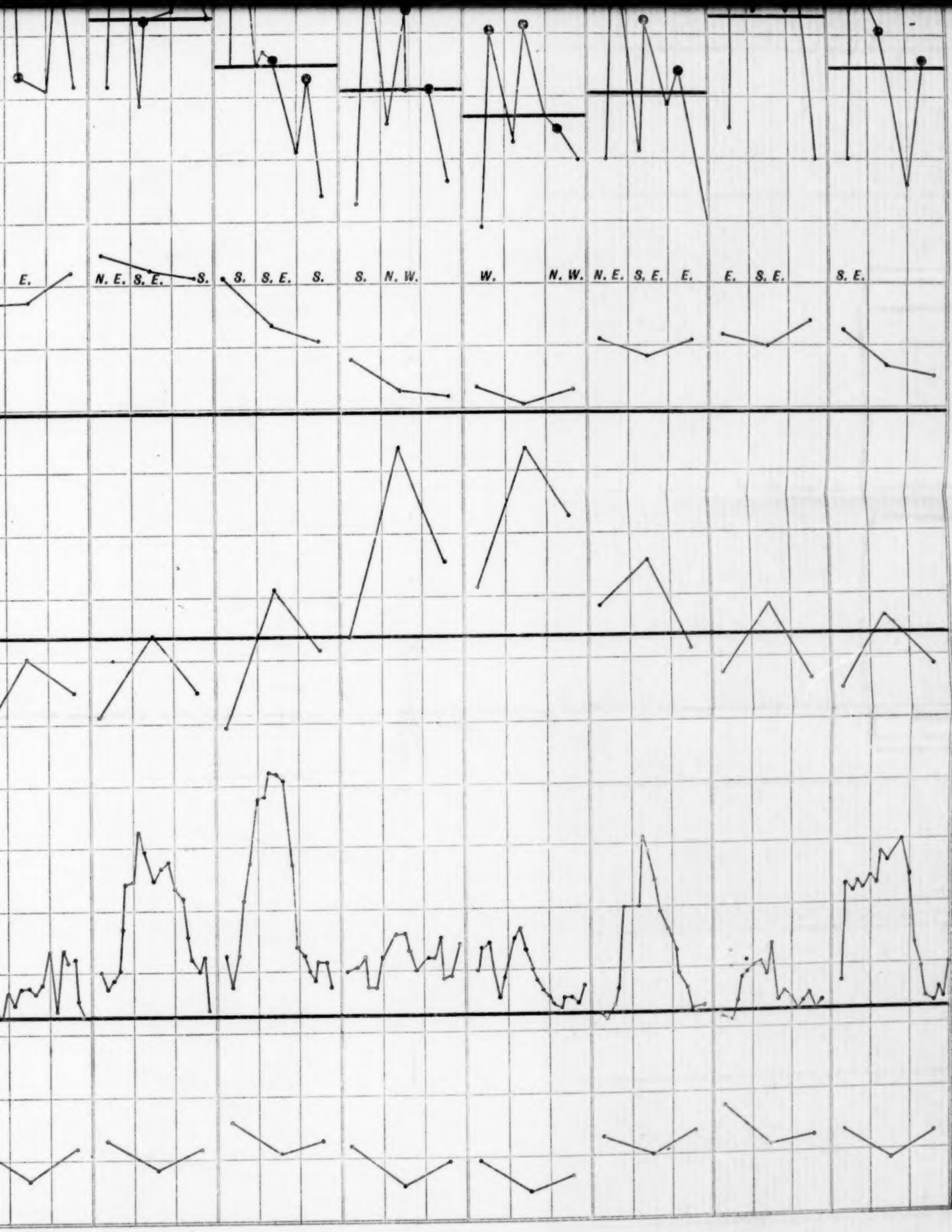
PLATE
EFFECT OF ALL THE DIFFERENT COMPONENTS
APRIL 1ST
APR. 2ND
APR. 3RD
APR. 4TH
APR. 5TH
APR. 6TH
APR. 7TH
12
6
80
70
60
50
40
30
20
10
Alterations in the Knee-jerk
Direction of Wind
N.
N. E.
W.
N. W.
N. W.
N.
N. W.
S. W.
S.
S.
S. W.
N. W.
N. W.
N. W.
S.
N.
E.
Barometric changes
40
30
20
10
30.00
90
80
70
Thermometric changes
70
60
50
40
30
Electrical changes of the air
300
200
100

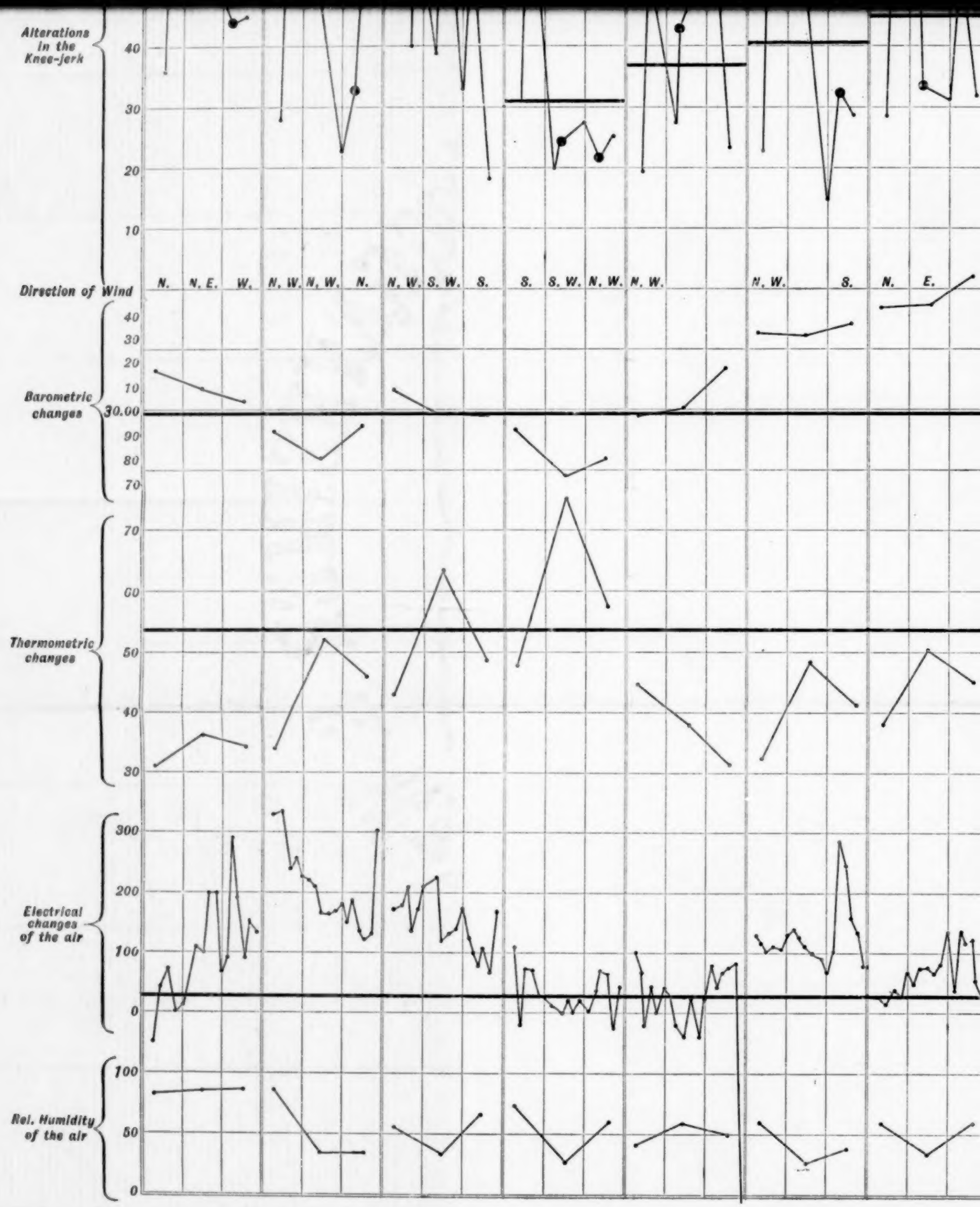
LATE II.

NENTS OF THE WEATHER UPON THE KNEE-JERK




Alterations in the Knee-jerk
Direction of Wind
N. N. E. W. N. W. N. W. N. N. W. S. W. S. S. S. W. N. W. N. W. N. W. S. N. E.
Barometric changes
30.00
Thermometric changes
Electrical changes of the air
Rel. Humidity of the air


E.
N. E. S. E.
S.
S. S. E. S.
S. N. W.
W.
N. W. N. E. S. E. E.
E. S. E.
S. E.



Alterations in the Knee-jerk
40
30
20
10
Direction of Wind
N.
N. E.
W.
N. W.
N. W.
N.
N. W.
S. W.
S.
S.
S. W.
N. W.
N. W.
N. W.
S.
N.
E.
Barometric changes
40
30
20
10
30.00
90
80
70
Thermometric changes
70
60
50
40
30
Electrical changes of the air
300
200
100
0
Rel. Humidity of the air
100
50
0

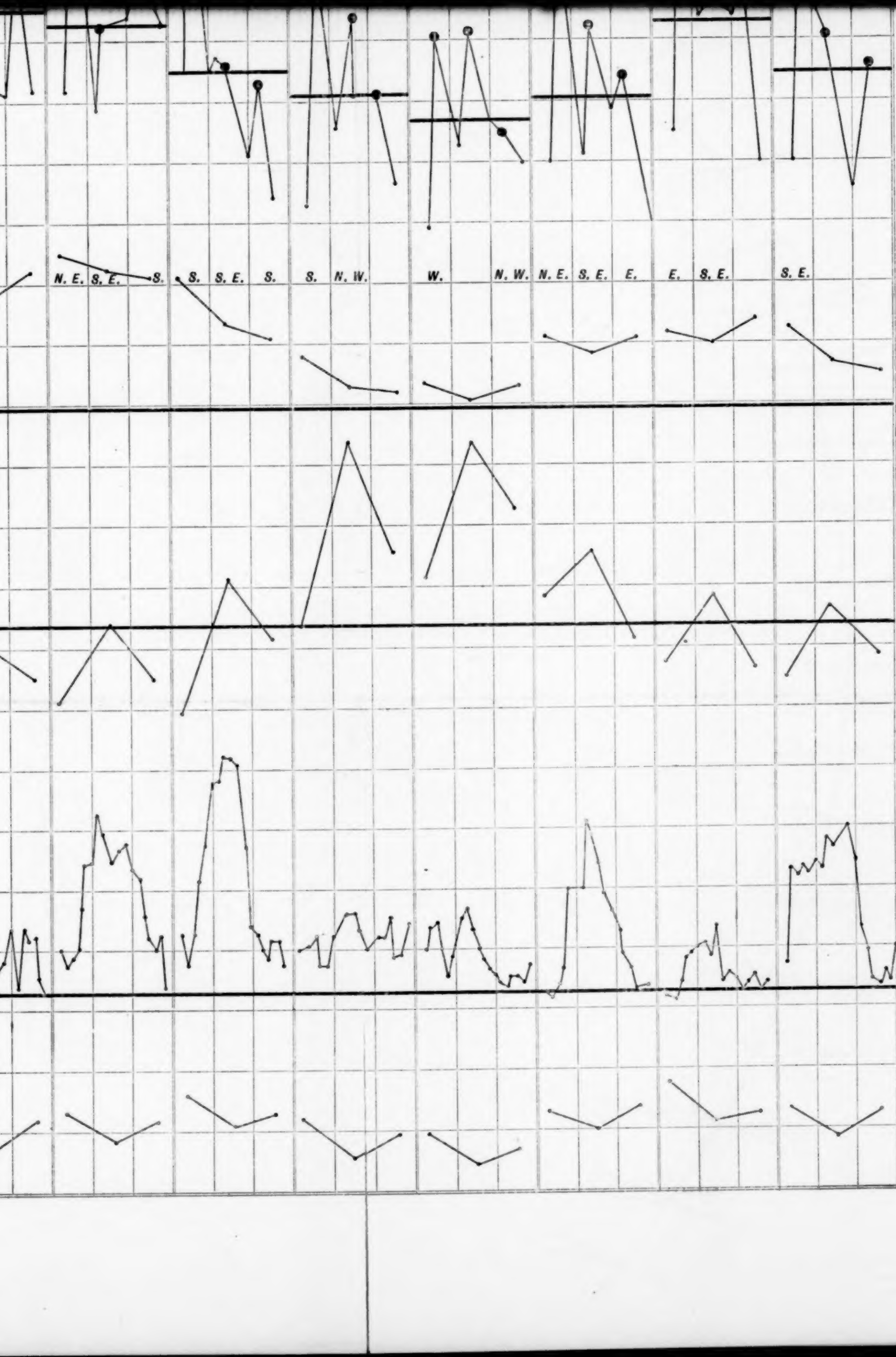

N. E. S. E. S.
S. S. E. S.
S. N. W.
W.
N. W. N. E. S. E. E.
E. S. E.
S. E.

PLATE III.